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THE MONAURAL LOCALIZATION OF SOUND.

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The present paper reports a series of observations upon the capacities of auditory localization in a person entirely deaf in one ear. In several of the experiments we have made parallel observations upon a person of normal hearing, and the cases are presented comparatively, in order to bring out as clearly as possible the peculiarities of monaural localization.

I.

We may conveniently preface the report of our experiments with a brief survey of the current theories of binaural localization.

Two general types of theory have been advanced to account for the ability to localize sounds. One of these, with which the names of Preyer and Münsterberg are associated, refers auditory localization to the action of the semicircular canals.¹ This view has as yet met with no general acceptance. The other type of theory, which is widely maintained by psychologists and physiologists, is formulated with admirable precision in the following words of the British scientist, S. P. Thompson:²

¹Preyer, *Pflüger's Archiv*, Bd. 40, S. 586. Münsterberg, *Beiträge z. Psychologie*, Heft II., S. 182. These writers differ distinctly in their conceptions of the precise *modus operandi* of the canals.

²S. P. Thompson, *Philosophical Magazine*, January-June, 1882, p. 415. This article contains an excellent critical discussion of the theories of Luca, Steinhauser, Graham Bell, Mayer, Mach, Lord Rayleigh and others. Among the best of recent investigations are those of Pierce, *PSYCHOLOGICAL REVIEW*, Vol. I., p. 461, and Matsumoto, *Yale Studies*, Vol. V., p. 1.

"Judgments as to the direction of sounds are based, in general, upon the sensations of different intensity in the two ears; but the perceived difference of intensity upon which a judgment is based, is not usually the difference in intensity of the lowest or fundamental tone of the compound sound (or 'clang'), but * * * the difference in intensity of the individual tone or tones of the clang for which the intensity-difference has the greatest effective result in the quality of the sound."

This statement obviously makes differences in the intensity and quality of the sound sensations perceived by the two ears the fundamental factors in our localizations. Many of the adherents of this general view emphasize the importance of the intensity element, as if that were really the basal fact, and mention difference in quality as though it were somewhat incidental and distinctly of secondary importance. Thompson's formulation puts the matter in an undoubtedly more accurate form. The significant objective difference in sound waves reaching the two ears with unequal intensity is not so much in the amplitude of the fundamental vibration, which gives the sound its pitch, as in the amplitude of the upper partials, which contribute very largely to the timbre or quality of an auditory complex.¹ Generally speaking, then, a difference in the intensity of the sound stimulations reaching the two ears results in the consciousness of a difference in the auditory quality of the two sensations, rather than in a mere perception of inequality in the intensity of the two stimulations. Of course this cannot be the case with pure tones. But it is to be remembered that pure tones are notoriously difficult to localize with any approach to accuracy. Our auditory orientation corresponds, therefore, to the intensive and qualitative difference in the sound sensations of the two ears, and of these the qualitative differences are seemingly much the more conspicuous in consciousness.

¹Thompson (*loc. cit.*) inclines to accept Lord Rayleigh's view that this modification of the partials is due chiefly to the effects upon the sound waves of the bones of the skull, as against Mach's contention that the pinnæ are mainly responsible for the phenomena. Our observations, as will be seen, tend on the whole to confirm Lord Rayleigh's theory on this point. The relative importance of the two factors varies with different objective positions of sounds, and both of them are at times clearly operative.

Unless modified by explanatory corollaries, a formulation phrased in this manner is unquestionably open to the criticism urged by Stumpf¹ and others who follow him. It is somewhat more precise than the earlier statements of v. Kries² and the similar formula of Matsumoto,³ quoted approvingly by Ladd.⁴ But at best it leaves much to be desired on the score of accuracy of expression. When dealing with intensity differences, the explanation which the statement advances requires a perception of the intensity of the sound in one ear as greater or less than the intensity of the sound in the other ear. Or, when expressed in terms of quality, it requires a perception of the quality of the sound in one ear different from the quality of the sound heard by the other ear. Neither statement corresponds exactly to the facts. We are conscious of only one sensation, only one intensity and only one quality. "This single sensation, with its intensity-quality attributes, is referred to some objective point in space. We may at times connect it with the predominant or exclusive action of one ear. But in no case is there any real comparison or analysis of the two intensities or qualities proceeding from the two ears.

Undoubtedly on some occasions the physical and physiological conditions resulting in the stimulation of the two ears with unequal intensities give rise to psychical processes involving localization of the sound. And undoubtedly, also, the physical and physiological conditions which are most often encountered are such as accompany differences in the quality of the sounds falling upon the two ears; with which, in turn, localizations may be connected. But this is a very different proposition from the one above quoted, with its implication of a conscious comparison of the intensities and qualities of sounds heard by the two ears.⁵

¹ Stumpf, *Tonpsychologie*, Vol. II., p. 50, ff.

² v. Kries, *Arch. f. Anat. und Physiol.* (Physiol. Abth.), 1877, p. 329. This author has, in a later publication (*Zeitsch. f. Psychol. und Physiol.*, Vol. I., p. 235) criticising Preyer and Münsterberg, made a much broader statement, including more explicit reference to the effects of quality upon auditory localization.

³ *Loc. cit.*

⁴ Ladd, *Outlines of Descriptive Psychology*, p. 191.

⁵ We omit discussion of sounds heard as double, either in binaural or monaural hearing. They do not involve the point we are here discussing. Cf. Stumpf, *loc. cit.*

We learn to localize sounds on the basis of differences among them, which are partly differences of intensity and partly differences of quality, involving sometimes variations in apparent pitch. These differences in intensity and quality have their physical and physiological basis largely in the differences between the stimulations of the two ears. But these physical and physiological differences are not reported in consciousness in the form of sensations connected with the separate ears. The theories which refer auditory localization immediately to these factors are, therefore, primarily engaged with the physical and physiological pre-conditions of the process.¹ Intensity and quality are unquestionably involved in conscious processes of auditory localization, to say nothing of motor and visual elements.² But these formulations with which we have been dealing are in point of fact only secondarily concerned with the strictly psychical phases of the phenomena. We shall take cognizance, in connection with our own work, of both kinds of facts.³

II.

We are now ready to begin the account of our experiments, and we shall anticipate at the outset our most important conclusion, in the belief that we may thus materially clarify our subsequent descriptions and discussions.

In the case of monaural hearing there can obviously be no question of the operation of the intensity factor in the form considered in the previous section. Provided the position of the head is fixed, changes in apparent intensity are, if unaccompanied by changes in quality, wholly ambiguous in monaural

¹ This procedure is much after the manner of the current color theories.

² Münsterberg's account (*loc. cit.*) of the part played in auditory localization by reflex and semi-reflex movements corresponds closely to the accepted view of the more careful psychological writers. This statement in no wise implies assent in such quarters to his theory of the dependence of these movements upon the action of the semicircular canals.

³ It is no part of the present undertaking to go into the merits of the nativistic-empiristic space controversy, and our investigations touch only indirectly, if at all, the problem of the intrinsic voluminousness and extensity of auditory sensations. Our immediate interest is in the fundamental physical and psychical conditions upon which monaural localization depends, and such localization is clearly in a space of the conventional visual-tactual-kinæsthetic kind and not in one of a purely auditory character.

hearing. They may mean change of distance, change of direction, change of actual objective intensity with or without change of either distance or direction, or finally any combination of these factors, and the subject has absolutely no reliable means of determining which of these alternatives is really involved in any specific case.

The qualitative differences already described in the quotation from Thompson are differences connected with variations in the stimulations of two ears. We find that qualitative differences in sounds coming from different directions and falling upon one ear are quite adequate to serve for localizations of considerable accuracy. Monaural hearing involves apparently a system of local signs based on qualitative differences, which are rapidly acquired for any given series of sounds, when not possessed at the time the sounds are first heard. This development is quite independent of any knowledge imparted through channels other than the auditory sensation itself, and independent of any assistance from the turning of the head, which might confirm, by changing the perceived intensity, the original judgment based on quality.

The subject of the experiments is a man of thirty, who lost the use of his left ear, when four years of age, through ulceration of the middle ear consequent upon an attack of scarlet fever. The tympanic membrane was practically destroyed and a scar has replaced it. How far the bones of the middle ear are affected it is not possible to state. The malleus appears, however, to be in place. The internal ear was probably attacked by the disease, of which all traces save deafness disappeared at the age of fifteen. In any case the deafness on the affected side seems to be complete. To assure ourselves of this as far as possible, the subject was examined by an experienced aurist. The Weber test applied with the C fork resulted in strong lateralizing in the intact ear. For both high and low tones complete deafness was demonstrated. The C₄ fork was heard when the intact ear was tightly closed. But it was heard with equal distinctness when the defective ear was also closed, showing that this ear had not participated in the transmission. The test for the duration of bone conduction could not be made,

because of the entire normality of one ear. The evidence points almost conclusively to lesion in the labyrinth.

The subject reports that, since the onset of the disease, he has never had the slightest reason to suppose he hears at all with this ear. The whole region of the external ear is, as compared with the intact side, rather numb in feeling. The normal ear is somewhat unusually acute. The intensive threshold is lower than that of the average person, and the sensibility for tone runs from 16 vibrations per second up to 35,000 per second. The musical capacity is good, with a limen for pitch discrimination rather better than the average. In no respect, however, is the hearing very remarkable.

The subject employed for comparison is entirely normal in every particular and requires, therefore, no further description.

Apparatus.—The apparatus used in these experiments was substantially the same as that used by Pierce and Matsumoto.¹ Our purpose was to give the subject sounds of the same quality and intensity from different directions. Equal intensity was secured by use of the same source of sound—in our experiments a telephone—placed always at the same distance from the head. To secure a uniform distance Pierce and Matsumoto used a spherical cage supported by standards, and so placed that the head of the subject, which was supported by a head-rest clamped to the back of his chair, should be in the center of the sphere. We found it more convenient to suspend our cage (diameter 4 ft.) from the gas-fixture by means of a wire. Using the wire as an axis of rotation, the experimenter could then stand always at the same place and, after attaching the telephone at the proper height, noiselessly turn the cage the required distance. Thus no suggestion of the location of the sound was given by the movements of the experimenter. In order to secure a constant position for the axis of rotation two uprights were fastened to the floor, and the equatorial circle of the sphere rested against them when the telephone-click was given. It should be added that we were thus able to reduce the framework of the sphere to two circles, the equatorial circle

¹ *Op. cit.*

and one vertical circle, the latter interrupted below to make room for the body of the subject.

In previous experiments upon auditory localization the subject was seated with the center of his head (the intersection of the visual axis with the line joining the two ears and named the auditory axis) in the center of the sphere. But since our subject was monaural it was at first thought necessary, in order that all stimuli might reach him with the same intensity, to center his intact ear (the right ear) rather than the head. This position was afterwards abandoned, for reasons to be mentioned later. It was also found convenient to nail a strip of wood to the floor, parallel with the front legs of the chair, and another strip in the same direction across the under side of the chair with the ends projecting. By placing his feet upon the first strip and his hands upon the projecting ends of the second, the subject could keep himself informed of the position of his body, in reference to which his localization was made. Without these aids his sense of bodily position seemed sometimes uncertain and seemed also to have a disturbing effect upon his localization of sounds.

We began by giving the sound twice for each test, but after the first series it was repeated often six to ten times, until the subject was satisfied with his judgment. The effect of this was simply to secure a normal condition of attention. The subject was then required to name the point from which the sound came, knowing of course the number and location of the points from which it might be given. The sound was given by the experimenter, who closed a circuit by means of a noiseless mercury-contact key. In some of the later experiments, however, it was found convenient to place a contact-key in the hands of the subject, allowing him to give the sound after being informed that the telephone was in position. This method would be recommended for future experiments as securing the best conditions of preparation. To avoid any suggestion of regularity in the order of the points given, the order for each day was determined in advance by shuffling cards bearing the names of the points.

Except where otherwise stated, the electric current used was from three Gonda cells. The current was passed through the primary circuit of an induction coil, in whose secondary circuit

was placed a d'Arsonval galvanometer. The galvanometer enabled us to verify the constancy of the current and the consequent constancy in the intensity of the telephone sound.

The method of notation used by Matsumoto appeared to be the most convenient. In his system the points are designated by significant letters. But we have not included in our system all the points used by him; and since the number of points from which the sound could come has an important relation to the numerical value of the error, we shall not be able to give satisfactory comparisons between his results and our own.

Our system of points was contained in three horizontal circles: an equatorial circle, whose plane passes through the center of the head, a circle 45° above the equatorial circle, and a third 45° below. There were in all sixteen points.

The points on the equatorial circle are designated as follows: *f* (front), the intersection of the visual axis of the subject with the circle in front of the subject; *b* (back), 180° from *f*; *r* (right), the intersection of the auditory axis with the circle on the right; *l* (left), 180° from *r*; *fr*, 45° from *f* and *r*; *br*, 45° from *b* and *r*; *fl*, 45° from *f* and *l*; *bl*, 45° from *b* and *l*.

The points on the upper circle are *fo*, *bo*, *ro*, and *lo*, *o* (over) denoting a point on the upper circle; those on the lower circle are *fu*, *bu*, *ru*, and *lu*, *u* (under) denoting a point on the lower circle.

Fifteen series of experiments were performed.¹ *Series I.* was for the purpose of determining the general characteristics of monaural as compared with binaural localization. A set of 480 experiments was made upon the monaural subject, *A*, and for purposes of comparison under approximately identical conditions a set of 232 was made upon *B*, a binaural subject. The order of each day's experiments was 24 upon *A*, then 24 upon *B*, followed by 24 more upon *A*. The results obtained from *A* are given in Table I., those from *B* in Table II.

The following points of comparison should be noted:

1. In the binaural localizations there were only two instances in which sounds in the median plane and the right and left hemispheres were confused with each other. Mats-

¹ The series are numbered in the order in which they were performed.

moto's subject made no errors of this kind. But 72 such errors are recorded for *A*, all of which, however, are confusions between the median plane and one of the two hemispheres; in no case are the hemispheres confused with each other.

2. The number of *A*'s correct judgments is 227 out of 480

TABLE I.

Position of sound.	f	fo	bo	b	bu	fu	fl	lo	bl	lu	l	fr	ro	br	ru	r	No. given.
f	17	6		2	1	1								3			30
fo		3	5	4								1	3	10		4	30
bo			20										2	5		3	30
b			1	25										4			30
bu				1	28				1								30
fu				18	2	1	1							7		1	30
fl			1	14	2		5	1	7								30
lo			3	2				9	15		1						30
bl								6	1	3	20						30
lu								1	12	2	15						30
l							1	2	2		25						30
fr												10	13			7	30
ro			2		1								14	6	1	6	30
br				1									1	22		6	30
ru					1									4	23	2	30
r														8		22	30
Total																	480

NOTE.—Heavy faced figures indicate right cases. Figures outside the larger squares indicate confusions of middle and right and left.

judgments, or 47.3%. *B*'s correct judgments were 168 out of 232 judgments, or 72.4%.

It will be of some interest to compare with these figures the results obtained by Matsumoto, though, as we have already noted, the difference in the number of points from which choices may be made renders the two results not exactly comparable; for the

points common to his system and ours he obtained out of 800 trials 483 right cases, or 60.4%.

3. The distribution of right judgments for *A* is: median plane 94, left hemisphere 42, right hemisphere 91. Noting that the median plane contains 6 points as against 5 in each hemi-

TABLE II.

Position of sound.	Direction in which the sound is perceived to lie.															No. given.
	f	fo	bo	b	bu	fu	fl	lo	bl	lu	l	fr	ro	br	ru	r
f	12	10														22
fo	1	15														16
bo		7	6													13
b				16	1											17
bu				7	15											22
fu		1	7	5	9	3										25
fl							12	4								16
lo								7			1					8
bl								3	10		1					14
lu									1	9						10
l								1			15					16
fr												12	1		1	14
ro													10			10
br				1										11		12
ru					1										7	8
r														1	8	9
Total																232

NOTE.—Heavy faced type shows right cases. Figures outside the larger squares indicate transpositions of middle and right and left.

sphere, the ratio of right judgments is as follows: middle 15.7, left 8.4, right 18.2; that is, about half as many right cases occur in the left hemisphere as in either the middle or right. It is to be remembered that the intact ear was on the right. The distribution of right cases for *B* was as follows: 58% of those

in the middle plane, 91 % of those on the left, and 83% of those on the right.

4. The points most often confused with each other were, for *A*, as follows: *l* and *bl* 22 (times), *lo* and *bl* 21, *b* and *fu* 18, *bl* and *lu* 15, *lu* and *l* 15, *fl* and *b* 14, *fr* and *ro* 13, *fo* and *br* 10. For *B* such points were: *f* and *fo* 11, *fu* and *bu* 9, *bu* and *b* 8, *fo* and *bo* 7. It should be noted that *B*'s chief confusions are between points in the median plane, while those of *A* are distributed generally. Since the number of experiments upon the two subjects was unequal, a numerical comparison is not intended: the figures are given merely to show the most important confusions of each subject in order of prominence.

5. These confusions may be further classified as follows: Transpositions forward, *A* 29 (times), *B* 9; transpositions backward, *A* 84, *B* 26. The tendency of both was to locate sounds backward rather than forward of their true position.

6. The following shows the number of errors, classified according to amount of error (disregarding direction). It should be noted that, in our method, the least ascertainable error was 45°. This undoubtedly over-emphasizes the actual error; but the only means of overcoming the difficulty promised more serious complications and was therefore disregarded.

	45°	90°	120°	135°	180°
<i>A</i>	164	26	21	40	2
<i>B</i>	35	17		5	7

The following shows the same, classified according to percentage of the whole amount of error:

	45°	90°	120°	135°	180°
<i>A</i>	64.8	10.3	8.3	15.8	.8
<i>B</i>	54.7	26.6		7.8	10.9

Series II. was performed in order to show the effect of practice. It seemed clear (the subject being in ignorance of the results obtained from himself) that *A* had improved rapidly in accuracy during the progress of Series I. A second series, of 271 experiments, was then given to ascertain the extent of this improvement. For convenience, five points (*bu*, *l*, *br*, *ru*, and *r*) which had already shown a large percentage of correct

cases were omitted in Series II. These points gave in Series I. 120 right and 30 wrong cases. Subtracting these amounts from the totals we have, for the remaining 11 points, 107 right and 223 wrong. The results for these points in Series II. were 139 right and 132 wrong, the number of experiments being about evenly distributed among the different points. A comparison of the two series shows, then, for *A*: in the first series 32.4% right, 67.6% wrong; in Series II., 51.3% right, 48.7% wrong. The distribution and general character of the errors was about the same as in Series I. No further experiments were made upon *B*.

Series III. and IV.—Having ascertained that *A*'s capacity for localization was in general not greatly inferior to that of binaural subjects, it became our object to determine, if possible, just what his data for localization were. It was suggested that he might be guided by cutaneous sensations from his head and neck. Accordingly, two series of experiments were performed in the endeavor to eliminate this factor. The head and neck were covered with a camera-cloth, in which a slit had been made to allow the right ear to protrude. In Series III. the camera-cloth was bound loosely to the head by a strap passed across the forehead and around the head. The results of this series appeared at first to give distinctly positive information. The number of right cases was only 109 out of 318 experiments, and not only were the hemispheres confused with the median plane, but left was a number of times transposed to right. The possibility remained, however, that the transpositions and other errors were due to the cutting off of partial tones, on which basis, perhaps, the localization was made, by the loose folds of the camera-cloth. Accordingly, another series (IV.) was tried, this time with the camera-cloth bound close to the head and face by means of straps passed around the head in three directions. The ratio of right cases (158 out of 310, or 51%) and the nature of the errors were neither of them materially different from those in series II. It seems probable, then, that cutaneous sensations constituted a small part, if any, in the data for localization, though it is necessarily uncertain how far the possible effects of the sound waves were checked by the camera-cloth.

It seems fairly certain, however, that the strong cutaneous stimulation afforded by the contact of the cloth with the skin must have overpowered any effects due to the sound waves alone.

In this connection we should mention a later series (IX.), which was performed with the same end in view. Both ears were fully closed with oily putty as far as the outer rim of the pinnae. The battery power was increased from three to five cells. 57 positions were then given, about evenly distributed among *f*, *b*, *r*, *l*. The only noticeable difference from the normal was the almost universal transposition of *f* to *b*.¹ Otherwise the localizations did not seem to be seriously affected. The net result of these three series was thus merely negative. It is possible, however, that interesting results in this direction might still be obtained from a subject absolutely deaf in both ears.

Series V.—A reference to Series I. and II. shows that sounds in the median plane were transposed frequently to the right but practically never to the left. The subject, *A*, who remained in ignorance of the results, also remarked that sounds which were (correctly) localized as *f* and *fl* appeared to come from a point somewhat to the right of the point which, from his knowledge of the system of points, he judged to be their true position. These facts seemed to indicate that he localized a sound with reference to his head rather than his ear. Accordingly, a series was tried in which the center of the head was located at the center of the sphere. The results are given in Table III.

Compared with Series I. and II., the following facts should be noted:

1. There were only 12 cases of confusion between the median plane and the hemispheres, as compared with 72 in Series I. and 33 in Series II. Comparing the number of experiments with Series I., there should be 48 such cases; with Series II., there should be 39. We have to do here undoubtedly in some measure with continued practice effects. It is, however, extremely improbable that practice is wholly accountable for the result.

¹Münsterberg (*op. cit.*) using a partial filling of wax for the external meatus and conch, decided that the pinnae were chiefly important in the localization of sounds from in front. Compare also note above on page 226.

It seems more reasonable to suppose that the conditions of this series were intrinsically more favorable for monaural localization.

2. The number of correct judgments is 240 out of 322, or 75%, as compared with 47.3% in Series I., 51.3% in Series II., 60.4%

TABLE III.

Position of sound.	Direction in which the sound is perceived to lie.																No. given.
	f	fo	bo	b	bu	fu	fl	lo	bl	lu	l	fr	ro	br	ru	r	
f	20																20
fo		16		7										1			24
bo			18													2	20
b				19										1			20
bu					19	1											20
fu	2			4		10								3			19
fl						1	16		3								20
lo		1						19									20
bl						1	17		0		2						20
lu									4	11	5						20
l							8	2	2		7						19
fr												19	1				20
ro													17		3		20
br	2													9		9	20
ru															20		20
r																20	20
Total																322	

NOTE.—Heavy faced type indicates right cases. Figures outside the larger squares indicate transpositions of the middle and right and left.

for Matsumoto's subject, and 72.4% for *B*. Taken as it stands the percentage is thus larger than any other recorded.

3. The distribution of right cases is: median plane, 102; right hemisphere, 85; left hemisphere, 53. Noting again that the number of points in the middle was 6, as compared to 5 in each hemisphere, the ratio is: median 17, right 17, left 11.

The greater proportion of error is naturally, as at the beginning, on the side of the deaf ear.

4. The main confusions are between *fl* and *bl*, 20; *fl* and *l*, 8; *br* and *r*, 9; *fo* and *b*, 7.

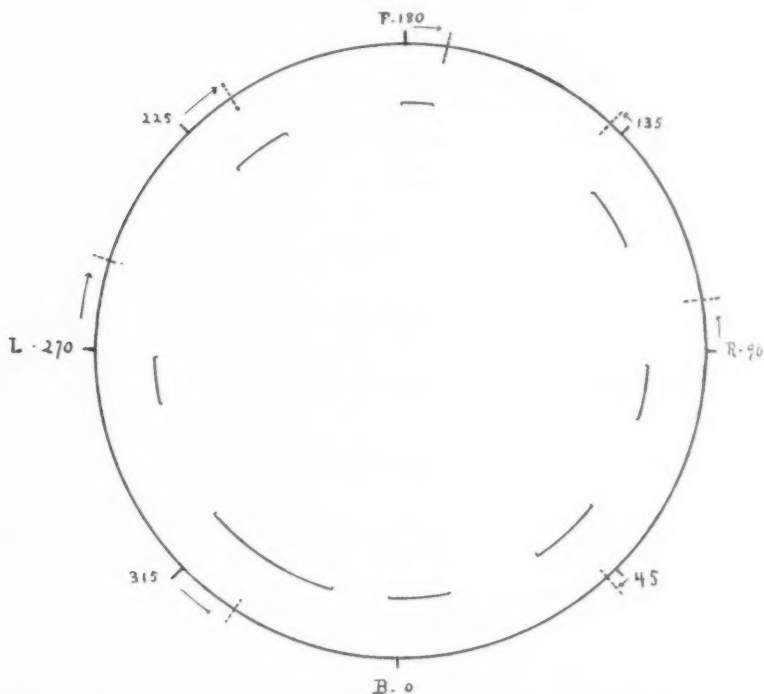
5. The most uncertain of all positions is *bl*, for which there are no right judgments. For some reason *l*, which was easily localized in Series I. and II., now partakes of the general uncertainty of all localizations in the left hemisphere. In this connection it should be noticed that *lu*, which has hitherto been very uncertain, now shows a fair percentage of right cases, the reason for which seemed to be that, in the first position of the body, *lu* was damped by the proximity of the subject's sleeve. This fact seems to have had a marked effect upon the apparent position of the sound. *fu*, which was close to the knees, was not at any time clearly identified, and was the only point seriously uncertain for *B*. These considerations point to the quality of the sound as a factor in localization.

Series VI. and VII. were merely confirmatory. Series VI. was for the purpose of eliminating as possible factors in localization the modifications of the sound due to the walls of the room and objects in the room. The subject had previously faced north; he now faced successively south, west and east, and in each position was given forty sounds, distributed equally among the four sound-positions most accurately localized (*f*, *r*, *b*, *bu*) and the four least accurately localized (*fu*, *l*, *bl*, *br*). A similar procedure was followed in Series VII., with the exception that here the variable factor was the intensity of the sound. The current had been furnished hitherto by three cells. In Series VII. the strength of the current was varied—two, three and five cells being used. Forty sounds were given with each arrangement of the cells—not, however, in separate groups, but in irregular order in one group, the subject not being informed of the strength of the current. No departure from the normal occurred in either series. This would again seem to indicate that the quality of the sounds, rather than the intensity of the fundamental tone in the sounds, was the important factor in the localization.

In *Series VIII.* we attempted to ascertain the location of the

eight points in the equatorial circle in the subjective auditory field of the subject *A*. Starting from a point on the equatorial circle about 45° from the point whose subjective location was required, the telephone was moved slowly toward the latter point, until the subject declared that it had been reached. The distance between the subjective and objective location of the point was then measured. Five measurements were made from each side of the point, and the mean of the averages of the two sets of five was chosen as the subjective location of the point. The locations were found to be as follows: f , 8.2° toward r ;

FIG. 1.



The dotted lines intersecting the outer circle indicate the places at which the subject localized the cardinal points on the equatorial circle. (For explanation, see Series VIII.)

The arcs of the incomplete inner circle indicate roughly the limits of variation in the subjective field of the sounds localized at the several cardinal points opposite the arcs. Thus, a sound localized at 90° was almost invariably felt as coming from a point somewhat back of 90° , although the amount of this subjective displacement varied, as the line shows.

fr, 2.6° toward *f*; *r*, 9.4° toward *f*; *br*, 2.2° toward *b*; *b* at *b*; *bl*, 12.5° toward *b*; *l*, 16° toward *f*; *fl*, 10.1° toward *f*. The mean variations for the sets of five were, in degrees, as follows: at *f* 2.2; at *fr* 2.5; at *r* 2.7; at *br* 2.7; at *b* 3.4; at *bl* 4.8; at *l* 2.6; at *fl* 1.7.

The accompanying diagram illustrates the facts brought out in this series, together with certain other peculiarities of the subjective field of auditory localization.

Series IX. has already been adequately described.

The remaining six series were for the purpose of testing definitely the hypothesis that *A*'s localization of sounds rested upon the modification, through the shape of the ear, head or body, of particular partial tones for particular directions. The hypothesis will be stated more fully later. Assuming it to be true, we should expect the accuracy of localization to vary with the complexity of the sound, with the pitch, and possibly with the distance. Accordingly, the remainder of the experiments was devoted to testing these points.

In *Series X.* we used the Galton whistle in place of the telephone. The tone employed was of approximately 10,700 vibrations per second. 92 positions were given, of which 29, or 31.5%, were localized correctly. From the median plane 3 were transposed to the left, 8 to the right; from the left 8 were transposed to the median plane, 3 to the right; from the right 3 were transposed to the median plane.

In *Series XI.* a tone of approximately 5,000 vibrations per second was used. 62 positions of the sound were given, of which 14, or 22.6%, were localized correctly. The transpositions were: from middle to left 6, to right 9; from left to middle 2; from right to middle 3.

In *Series XII.* the whistle tone was lowered to approximately 2,500 vibrations per second. 46 positions were given, of which 32, or 70%, were correctly localized; no new transpositions appeared.

The tone of the Galton whistle is not pure, but save in the case of the deeper tones which it is capable of giving, *e. g.*, that used in *Series XII.*, the partial tones are weak and very high. The complex nature of the tone is, therefore, not easily

detected. We should expect, then, that the localizations of its higher tones would be more uncertain than those of the telephone. A reference to Series X. and XI. shows this to be the fact. Comparison may be made with either Series I. or V.

We should expect Series X. to show a smaller percentage of correct localizations than Series XI., as qualitative differences ought to be more difficult of detection, because the partial tones should be higher and weaker. The percentage of correct localizations is, however, slightly in favor of Series X. This fact is more than offset by the nature of the confusions and transpositions which are encountered in Series X. The hemispheres are confused with each other, as well as with the median plane. This does not occur in Series XI. The total number of tests is too small to warrant great insistence upon this difference. But the marked difference between Series X. and XI. on the one hand and Series XII. on the other suggests strikingly the effect of using sounds whose partial tones are weak and very high, as compared with those possessing partials which are relatively strong and well within the limits of audition. Series XII. shows almost as good results as Series V.

In *Series XIII.* we tried to discover whether the peculiar quality of the sound supposed to be the basis of its localization was a function of its distance from the ear. Accordingly, the eight equatorial positions were given each four times at a point thirty inches beyond the surface of the sphere, the telephone being used as the source of sound. The results were the same, however, as the normal results given in Series V. We can hardly conclude from this that greater variations of distance would be equally ineffective.

In *Series XIV.* the sound was given with a tuning-fork of 1,024 vibrations. The fork was placed at the surface of the sphere and only the eight equatorial positions were given. 42 sounds were given, of which 6, or 14.3 %, were correctly localized; the latter were one each at *f*, *fr*, *l* and *fl*, and two at *r*. In 10 cases the subject was unable to localize the sound at all.

In *Series XV.* we substituted a fork of 512 vibrations. 24 sounds were given, of which none were correctly localized;

20 were incorrectly localized and 4 could not be localized. Similar results were obtained with a fork of 256 vibrations.

The tuning-fork tones of Series XIV. and XV. are theoretically pure tones. As a matter of fact it is difficult to avoid, in striking the forks, some slight production of partial tones. This is more noticeable with the 1,024 fork than with the 512 and 256. For the two lower forks localization is clearly out of the question. In the case of the 1,024 fork, also, it is essentially impossible. The number of localizations correctly made in the latter case is somewhat above the number to be expected by the laws of chance, but it is to be added that the subjective attitude even in these correct cases was one of extreme uncertainty, and the total number of tests made is too small to permit of attaching much importance to the operations of mere chance. With the tuning-forks, and in less degree with all of the sounds we used, the change in position is accompanied by slight apparent changes in pitch. In the case of the telephone and whistle tests this change is partially, at least, incidental to the modification of the partial tones. In the case of the tuning-forks the cause is probably of another character, but whatever its nature it is inadequate to afford reliable localizations under the conditions we have studied.

III.

Taking account of all the facts brought out by the tests, we reach the following conclusions:

1. The differences in the localizing capacity for complex sounds in binaural and monaural hearing are, so far as concerns these subjects, interpretable as chiefly differences in the magnitude of the difference limen for locality, rather than as absolute differences in the kind of localizing process involved. In monaural hearing the localization of sounds is by no means a matter of chance. On the side of the intact ear the localizations are in no sense seriously defective, and the distinction between front and back may be made in a manner superior to that of binaural hearing. (Cf. Series I., II. and V.) In the region opposite the deaf ear the localizations are extremely uncertain, but

elsewhere the errors met with are simply slight exaggerations of those made in binaural hearing.

It is not intended to imply in this statement that monaural localization is generally as prompt as binaural, nor that it is as accurate in its capacities for dealing with new and strange sounds. (Compare Series I. with Series II. and V. showing practice effects.) But practically all complex sounds are localized with a reasonable degree of accuracy, subject to the exceptions mentioned in connection with Series X., XI., XII., even upon a first hearing, and a little experience with the sound, familiarizing the subject with its quality changes, leads to such accuracy in its localization as is exhibited in Series V.

- * 2. The experiments objectively considered amply sustain the introspection of the subject in pointing to qualitative differences in the sounds coming from different directions, as the basis of the localizations. This was explicitly commented upon by the subject very early in the course of the experimentation.

Sounds which are complex in nature undoubtedly undergo modification through the damping and reinforcing of their partial tones by the pinnae, the external meatus and the head, in a manner which must vary somewhat regularly with variations in the spatial position of the object from which the sound emanates. When the changes in these objective positions are small, the corresponding changes in the quality of the sounds are ordinarily minute. But there is undoubtedly a general parallelism between the two series of events, and there is no reason why these variations should not be employed for auditory localizations as genuinely in monaural hearing as in binaural hearing, albeit necessarily with less of accuracy in some portions of the field.

The one region throughout which in monaural hearing such differences in the quality of sounds would be absent, or seriously ambiguous, is that immediately opposite the defective ear. In this case the whole of the head is interposed between the source of the sound and the intact ear. Sounds are much weakened in absolute intensity by this circumstance, as anyone may speedily convince himself, and the damping of the partial tones, upon which depend the qualitative peculiarities of sounds,

is essentially similar for all points in the region.¹ The extreme uncertainty and the persistent confusions which we encounter in monaural localization throughout this region are then precisely what we should anticipate, provided the qualitative differences of sounds constitute the basis of such localization.

Certainly it is striking that those sounds are most accurately and most promptly localized, which contain a considerable number of partial tones well within the limits of audition. The more nearly the sounds approach pure tones, the more inaccurate the localization. This is true regardless of the absolute pitch. Genuinely pure tones are essentially unlocalizable in monaural hearing. Such localization of these tones as is possible in binaural hearing is, therefore, probably referable to the intensity factor.² It is to be remembered that the deeper tones which we employed are practically pure. Most of the higher tones possess a few overtones. The sounds most accurately localized were always those in which qualitative differences were noticed and in which modification of partial tones was physically possible under the conditions.

It is assumed, in this statement, that the effects of experience, involving especially motor and visual processes, enter indispensably into acts of auditory localization. With adults, these intensive-qualitative auditory differences, to which we have made constant reference, simply furnish the relatively fixed psychic contents, whose spatial relations can then be learned. The process of giving to these elements values in the general visual-tactual-kinæsthetic space world is definitely the business of experience. How far some of these motor processes may be connected in a genuinely reflex and hereditary way with auditory stimulations is not a matter which we can at present discuss.

¹ The failure of some persons deaf in one ear to localize with any considerable degree of accuracy may be due to any one of a number of reasons, no one of which would impeach the validity of our interpretation.

² This radical defect in the monaural localization of pure tones furnishes a serious difficulty to those theories which refer auditory localization to the reflex action of the semi-circular canals. One can conceive of reasons why, as in binaural hearing, such localization should be uncertain and relatively inaccurate. But it is not so easy to explain how in monaural localization the process should work fairly well for many sounds, only to break down hopelessly upon pure tones.

Our examination of monaural hearing suggests, however, the extent to which one set of auditory symbols, *i. e.*, qualitative peculiarities in sounds, may serve for localizations. It substantiates indirectly, therefore, those theories of binaural localization which emphasize quality changes as more important for consciousness than mere variations in intensity.

It may be suggested that the changes in the quality of sounds, which seem to furnish the basis of monaural localization, are in one particular seriously ambiguous. Changes in distance, as well as changes in direction, are frequent causes of an apparent change in auditory quality. This being true, it would seem to indicate that in monaural hearing these two changes would often be confused with each other.

Undoubtedly variations in the quality of sounds may be produced by changes of either kind. But it is by no means certain that the effects upon the quality of the two kinds of change are sufficiently similar to be often confusing. Certainly our tests in Series XIII., although by no means conclusive, suggest strongly the ability to distinguish such qualitative-intensive differences as are due to changes in direction, from such as are due to changes of distance.

3. The presence of eye-reflexes was often very marked, and the final localization was frequently made on the basis of a seeming correspondence between the eye-strains and the supposed direction of the sound. This statement leaves wholly untouched the physiological basis of the eye-movements. It may be added, however, that the subject is rather a vivid visualizer and almost always thinks of spatial relations in visual form.

4. There is no good evidence for supposing that cutaneous sensations played any part in the localizations.

THE INFLUENCE OF IMPROVEMENT IN ONE
MENTAL FUNCTION UPON THE
EFFICIENCY OF OTHER
FUNCTIONS. (I.)

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This is the first of a number of articles reporting an inductive study of the facts suggested by the title. It will comprise a general statement of the results and of the methods of obtaining them, and a detailed account of one type of experiment.

The word function is used without any rigor to refer to the mental basis of such things as spelling, multiplication, delicacy in discrimination of size, force of movement, marking *a*'s on a printed page, observing the word *boy* in a printed page, quickness, morality, verbal memory, chess playing, reasoning, etc. Function is used for all sorts of qualities in all sorts of performances from the narrowest to the widest, *e. g.*, from attention to the word 'fire' pronounced in a certain tone, to attention to all sorts of things. By the word improvement we shall mean those changes in the workings of functions which psychologists would commonly call by that name. Its use will be clear in each case and the psychological problem will never be different even if the changes studied be not such as everyone would call improvements. For all purposes 'change' may be used instead of 'improvement' in the title. By efficiency we shall mean the status of a function which we use when comparing individuals or the same individual at different times, the status on which we would grade people in that function. By other function we mean any function differing in any respect whatever from the first. We shall at times use the word function-group to mean

those cases where most psychologists would say that the same operation occurred with different data. The function *attention*, for instance, is really a vast group of functions.

2. Our chief method was to test the efficiency of some function or functions, then to give training in some other function or functions until a certain amount of improvement was reached, and then to test the first function or set of functions. Provided no other factors were allowed to affect the tests, the difference between the test before and the test after training measures the influence of the improvement in the trained functions on the functions tested.

It is possible to test the general question in a much neater and more convenient way by using, instead of measures of a function before and after training with another, measures of the correlation between the two functions. If improvement in one function increases the efficiency of another and there has been improvement in one, the other should be correlated with it; the individuals who have high rank in the one should have a higher rank in the other than the general average. Such a result might also be brought about by a correlation of the inborn capacities for those functions. Finding correlation between two functions thus need not mean that improvement in one has brought increased efficiency in the other. But the absence of correlation does mean the opposite. In an unpublished paper Mr. Clark Wissler, of Columbia University, demonstrates the absence of any considerable correlation between the functions measured by the tests given to students there. Miss Naomi Norsworthy, of Teachers College, has shown (the data were presented in part at the Baltimore meeting; the research is not yet in print) that there is no correlation between accuracy in noticing misspelled words and accuracy in multiplication, nor between the speeds; that there is little or no correlation between accuracy and speed in marking on a printed page misspelled words, words containing *r* and *e*, the word *boy*, and in marking semi-circles on a page of different geometrical figures.

Perhaps the most striking method of showing the influence or lack of influence of one function on another is that of testing the same function-group, using cases where there are very

slightly different data. If, for instance, we test a person's ability to estimate a series of magnitudes differing each from the next very slightly, and find that he estimates one very much more accurately than its neighbors on either side, we can be sure that what he has acquired from his previous experience or from the experience of the test is not improvement in the function-group of estimating magnitudes but a lot of particular improvements in estimating particular magnitudes, improvements which may be to a large extent independent of each other.

2. The experiments, finally, were all on the influence of the training on efficiency, on ability as measured by a single test, not on the ability *to improve*. It might be that improvement in one function might fail to give in another improved ability, but succeed in giving ability to improve faster than would have occurred had the training been lacking.

The evidence given by our experiments makes the following conclusions seem probable :

It is misleading to speak of sense discrimination, attention, memory, observation, accuracy, quickness, etc., as multitudinous separate individual functions ^{consisting of} ~~are~~ referred to by any one of these words. These functions may have little in common. There is no reason to suppose that any general change occurs corresponding to the words 'improvement of the attention,' or 'of the power of observation,' or 'of accuracy.'

It is even misleading to speak of these functions as exercised within narrow fields as units. For example, 'attention to words' or 'accurate discrimination of lengths' or 'observation of animals' or 'quickness of visual perception' are mythological, not real entities. The words do not mean any existing fact with anything like the necessary precision for either theoretical or practical purposes, for, to take a sample case, attention to the meaning of words does not imply equal attention to their spelling, nor attention to their spelling equal attention to their length, nor attention to certain letters in them equal attention to other letters.

The mind is, on the contrary, on its dynamic side a machine for making particular reactions to particular situations. It works in great detail, adapting itself to the special data of which

it has had experience. The word *attention*, for example, can properly mean only the sum total of a lot of particular tendencies to attend to particular sorts of data, and ability to attend can properly mean only the sum total of all the particular abilities and inabilities, each of which may have an efficiency largely irrespective of the efficiencies of the rest.

4. Improvement in any single mental function need not improve the ability in functions commonly called by the same name. It may injure it.

Improvement in any single mental function rarely brings about equal improvement in any other function, no matter how similar, for the working of every mental function-group is conditioned by the nature of the data in each particular case.

The very slight amount of variation in the nature of the data necessary to affect the efficiency of a function-group makes it fair to infer that no change in the data, however slight, is without effect on the function. The loss in the efficiency of a function trained with certain data, as we pass to data more and more unlike the first, makes it fair to infer that there is always a point where the loss is complete, a point beyond which the influence of the training has not extended. The rapidity of this loss, that is, its amount in the case of data very similar to the data on which the function was trained, makes it fair to infer that this point is nearer than has been supposed.

5. The general consideration of the cases of retention or of loss of practice effect seems to make it likely that spread of practice occurs only where identical elements are concerned in the influencing and influenced function.

The particular samples of the influence of training in one function on the efficiency of other functions chosen for investigation were as follows:

1. The influence of certain special training in the estimation of magnitudes on the ability to estimate magnitudes of the same general sort, *i. e.*, lengths or areas or weights, differing in amount, in accessory qualities (such as shape, color, form) or in both. The general method was here to test the subject's accuracy of estimating certain magnitudes, *e. g.*, lengths of lines. He would, that is, guess the length of each. Then he would

practice estimating lengths within certain limits until he attained a high degree of proficiency. Then he would once more estimate the lengths of the preliminary test series. Similarly with weights, areas, etc. This is apparently the sort of thing that happens in the case of a tea-taster, tobacco-buyer, wheat-taster or carpenter, who attains high proficiency in judging magnitudes or, as we ambiguously say, in delicacy of discriminating certain sense data. It is thus like common cases of sense training in actual life.

2. The influence of training in observing words containing certain combinations of letters (*e. g.*, *s* and *e*) or some other characteristic on the general ability to observe words. The general method here was to test the subject's speed and accuracy in picking out and marking certain letters, words containing certain letters, words of a certain length, geometric figures, misspelled words, etc. He then practiced picking out and marking words of some one special sort until he attained a high degree of proficiency. He was then re-tested. The training here corresponds to a fair degree with the training one has in learning to spell, to notice forms and endings in studying foreign languages, or in fact in learning to attend to any small details.

3. The influence of special training in memorizing on the general ability to memorize. Careful tests of one individual and a group test of students confirmed Professor James' result (see *Principles of Psychology*, Vol. I., pp. 666-668). These tests will not be described in detail.

These samples were chosen because of their character as representative mental functions, because of their adaptability to quantitative interpretations and partly because of their convenience. Such work can be done at odd times without any bulky or delicate apparatus. This rendered it possible to secure subjects. In all the experiments to be described we tested the influence of improvement in a function on *other functions closely allied to it*. We did not in sense-training measure the influence of training one sense on others, nor in the case of training of the attention the influence of training in noticing words on, say, the ability to do mental arithmetic or to listen to a metaphysical discourse. For common observation seemed to give a negative

answer to this question, and some considerable preliminary experimentation by one of us supported such a negative. Mr. Wissler's and Miss Norsworthy's studies are apparently conclusive, and we therefore restricted ourselves to the more profitable inquiry.

A SAMPLE EXPERIMENT.

There was a series of about 125 pieces of paper cut in various shapes. (Area test series.) Of these 13 were rectangles of almost the same shape and of sizes from 20 to 90 sq. cm. (series 1), 27 others were triangles, circles, irregular figures, etc., within the same limits of size (series 2). A subject was given the whole series of areas and asked to write down the area in sq. cm. of each one. In front of him was a card on which three squares, 1, 25 and 100 sq. cm. in area, respectively, were drawn. He was allowed to look at them as much as he pleased but not to superpose the pieces of paper on them. No other means of telling the areas were present. After being thus tested the subject was given a series of paper rectangles,¹ from 10 to 100 sq. cm. in area and of the same shape as those of series 1. These were shuffled and the subject guessed the area of one, then looked to see what it really was and recorded his error. This was continued and the pieces of paper were kept shuffled so that he could judge their area only from their intrinsic qualities. After a certain amount of improvement had been made he was re-tested with the 'area test series' in the same manner as before.

¹The judgments of area were made with the following apparatus: a series of parallelograms ranging from 10 to 140 and from 190 to 280 sq. cm., varying each from the next by 1 sq. cm. Their proportions were almost the same (no one of them could possibly be distinguished by its shape). For example, the dimensions of those from 137 to 145 sq. cm. were

15	× 9.133½
15	× 9.2
15.1	× 9.2 +
15.1	× 9.275
15.15	× 9.31
15.15	× 9.375
15.2	× 9.41
15.2	× 9.475
15.2	× 9.54

The function trained was that of estimating areas from 10 to 100 sq. cm. with the aid of the correction of wrong tendencies supplied by ascertaining the real area after each judgment. We will call this 'function *a*.' A certain improvement was noted. What changes in the efficiency of closely allied functions are brought about by this improvement? Does the improvement in this function cause equal improvement (1) in the function of estimating areas of similar size but different shape without the correction factor? or (2) in the function of estimating identical areas without the correction factor? (3) In any case how much improvement was there? (4) Is there as much improvement in the function of estimating dissimilar shapes as similar? The last is the most important question.

We get the answer to 1 and part of 3 by comparing in various ways the average errors of the test areas of dissimilar shape in the before and after tests. These are given in Table I. The average errors for the last trial of the areas in the training series similar in size to the test series are given in the same table.

TABLE I.

Subject.	Test series 2.		Training series. Av. error at end of training.
	Av. error before training.	Av. error after training.	
T	15.8	11.1	2.3
Be.	28.0	5.2	3.1
Br.	22.5	18.7	3.3
J. W.	12.7	21.0	1.5 approx.
W. (2)	17.0	20.0	4.0 "
E. B.	10.5	7.9	0.4

The function of estimating series 2 (same sizes, different shapes) failed evidently to reach an efficiency equal to that of the function trained. Did it improve *proportionately* as much?

This is a hard question to answer exactly, since the efficiency of 'function *a*' increases with great rapidity during the first score or so of trials, so that the average error of even the first twenty estimates made is below that of the first ten, and that again is below that of the first five. Its efficiency at the start depends thus on what you take to be the start. The fact is that the first estimate of the training series is not an exercise of 'function *a*' at all and that the *correction* influence increases

up to a certain point which we cannot exactly locate. The fairest method would seem to be to measure the improvement in 'function *a*' from this point and compare with that improvement the improvement in the other function or functions in question. This point is probably earlier in the series than would be supposed. If found, it would probably make the improvement in 'function *a*' greater than that given in our percentages.

The proportion of average error in the after test to that in the before test is greater in the case of the test series than in the case of the first and last estimations of the areas of the same size in the training series, save in the case of *Be*. The proportions are given in the following table:

TABLE II.

Proportion of error after to error before training.		
Subject.	Test series 1.	Training series.
T.	.70	.575
Be.	.19	.56
Br.	.83	.53
J. W.	1.75	.77 approx.
W (2)	1.18	.83 approx.
E. B.	.75	.13

Question 2 is answered by a comparison of the average errors, before and after the training, of Series I. (identical areas) given without the correction factor. The efficiency reached in estimating without the correction factor (see column 2 of Table III.) is evidently below that reached in 'function *a*.' The results there in the case of the same areas are given in column 3.

TABLE III.

Subject.	Av. error before training of series 1.	Av. error after training of series 1.	Av. error after training of same sizes in training series.	Av. error after training of series 2.	Proportion of error after to error before training.		
					Series 2.	Series 1.	Areas of training series identical with series 1.
T.	9.0	6.0	2.1	11.1	.70	.67	.31
Be.	21.9	6.4	1.8	5.2	.19	.29	.45
Br.	24.2	14.7	3.7	18.7	.83	.61	.37
J. W.	7.7	8.6	1.5 app.	21.0	1.75	1.11	.77 app.
W. (2)	11.6	3.3 app.	4.0 app.	20.0	1.18	.28 app.	.83 app.
E. B.	9.8	4.1	0.4	7.9	.75	.42	.08

The function of estimating an area while in the frame of mind due to being engaged in estimating a limited series of areas and seeing the extent of one's error each time, is evidently independent to a large extent of the function of judging them after the fashion of the tests.

If we ask whether the function of judging without correction improved proportionately as much as 'function *a*,' we have our previous difficulty about finding a starting point for *a*. Comparing as before the first 100 estimates with the last 100 we get the proportions in the case of the areas identical with those in the test. These are given in column 7. The proportions in the case of the test areas (series 1; same shape) are given in column 6. A comparison of columns 6 and 7 thus gives more or less of an answer to the question, and column 6 gives the answer to the further one: "How much improvement was there?"

We can answer question 4 definitely. Column 5 repeats the statement of the improvement in the case of the test areas of different shape, and by comparing column 6 with it we see that in every case save that of Be. there was more improvement when the areas were similar in shape to those of the training series. This was of course the most important fact to be gotten at.

To sum up the results of this experiment, it has been shown that the improvement in the estimation of rectangles of a certain shape is not equalled in the case of similar estimations of areas of different shapes. The function of estimating areas is really a function-group, varying according to the data (shape, size, etc.). It has also been shown that even after mental standards of certain limited areas have been acquired, the function of estimating with these standards constantly kept alive by noticing the real area after each judgment is a function largely independent of the function of estimating them with the standards fully acquired by one to two thousand trials, *but not constantly renewed by so noticing the real areas*. Just what happened in the training was the partial formation of a number of associations. These associations were between sense impressions of particular sorts in a particular environment coming to a person in a particular mental attitude or frame of mind, and a number of ideas or impulses.

What was there in this to influence other functions, other processes than these particular ones? There was first of all the acquisition of certain improvements in mental standards of areas. These are of some influence in judgments of different shapes. We think, "This triangle or circle or trapezoid is about as big as such and such a rectangle, and such a rectangle would be 49 sq. cm." The influence is here by means of an idea that may form an identical element in both functions. Again, we may form a particular habit of making a discount for a tendency to a constant error discovered in the training series. We may say, "I tend to judge with a minus error," and the habit of thinking of this may be beneficial in all cases. The habit of bearing this judgment in mind or of unconsciously making an addition to our first impulse is thus an identical element of both functions. This was the case with *Be*. That there was no influence due to a mysterious transfer of practice, to an unanalyzable property of mental functions, is evidenced by the total lack of improvement in the functions tested in the case of some individuals.

* On pushing our conception of the separateness of different functions to its extreme, we were led to ask if the function of estimating one magnitude might not be independent even of the functions of estimating magnitudes differing only slightly from the first. It might be that even the judgment of areas of 40-50 sq. cm. was not a single function, but a group of similar functions, and that ability might be gained in estimating one of these areas without spreading to the others. The only limits that must necessarily be set to this subdivision would be those of the mere sensing of small differences.

If, on the contrary, judgments of nearly equal magnitudes are acts of a single function, ability gained in one should appear in the others also. The results of training should diffuse readily throughout the space covered by the function in question, and the accuracy found in judgments of different magnitudes within this space should be nearly constant. The differences found should simply be such as would be expected from chance.

The question can be put to test by comparing the actual difference between the average errors made, in judging each of

neighboring magnitudes, with the probable difference as computed from the probability curve. If the actual difference greatly exceeds the probable difference, it is probably significant of some real difference in the subject's ability to judge the two magnitudes. He has somehow mastered one better than the other. No matter how this has come about. If it is a fact, then clearly ability in the one has not been transferred to the other.

Our experiments afford us a large mass of material for testing this question. In the 'training series,' we have a considerable number (10 to 40) of judgments of each of a lot of magnitudes differing from each other by slight amounts. We have computed the accuracy of the judgment of each magnitude (as measured by the error of mean square), and then compared the accuracy for each with that for the adjacent magnitudes. We find many instances in which the difference between the errors for adjacent magnitudes is largely in excess of the probable difference. And the number of such instances greatly exceeds what can be expected from chance.¹

These great differences between the errors of adjacent magnitudes are strikingly seen in the curves on page 259. The ordinates of these curves represent the mean square error of judgments of areas of 10 to 100 square centimeters, and for 3 individuals. The dots above and below each point of the curve give the 'limits of error' of that value, as determined by the

formula, $\frac{\mu}{\sqrt{2n}}$, in which μ is the error of mean square, and n

the number of cases. These limits are such that the odds are about 2 to 1, more exactly 683 to 317, that the true value lies inside them. The dots thus furnish a measure of the reliability of the curve at every point.

¹ The smaller error at certain magnitudes is not the result of a preference of the subject to guess that number. Of course, if the subject were prone to guess '64 square centimeters' oftener than 63 or 65, he would be more apt to guess 64 right, and the error for 64 would be diminished. We therefore made a few tables of the frequency with which each number was guessed. But we found that the magnitudes that were best judged were not more often guessed than their neighbors.

These curves are all irregular, with sudden risings and fallings that greatly obscure their general course. Psychologists are familiar of old with irregularities of this kind, and are wont to regard them as effects of chance, and so to smooth out the curve. But as we find more irregularity than can reasonably be attributed to chance, we conclude that our curves at least should not be smoothed out, and that the sudden jumps, or some of them, signify real differences in the person's ability.¹

If, for example, we examine Fig. 1, we notice a number of sudden jumps, or points at which the errors in judging adjacent magnitudes differed considerably from each other. The most significant of these jumps are at 10-11, 36-37, 41-42, 65-66, 66-67, 83-84, and 98-99 sq. cm. The question is whether such a jump as that at 41-42 indicates greater ability to judge 42 sq. cm., or whether the observed difference is simply due to chance and the relatively few cases (here 10 for each area). A vague appeal to chance should not be allowed, in view of the possibility of calculating the odds in favor of each side of the question. This can be done by a fairly simple method. We can consider two adjacent areas as practically equal, so far as concerns Weber's law or any similar law. The average errors found for the two would thus be practically two determinations of the same quantity, and should differ only as two determinations of the same quantity may probably differ.

We wish then to compare the actual difference between the errors for 41 and 42 sq. cm. with the probable difference. The error—we use throughout the 'error of mean square,' and the measure of reliability based on it—this error is here 6.2 and 3.1 sq. cm. respectively. The actual difference is 3.1 sq. cm. To find the probable difference, we first find the 'limits of error' or reliability of each determination, as described above, and then find the square root of the sums of the squares of these

¹ The fact that judgments of nearly equal magnitudes may show very unequal errors throws doubt on all curves drawn from the judgment of only a few 'normals.' If slightly different normals had been chosen, the errors might have been considerably different, and the course of the curve changed. If, for example, three normals be chosen from the 91 in our curves, and those three used as the basis of a curve, the curve will vary widely with the choice of the three normals.

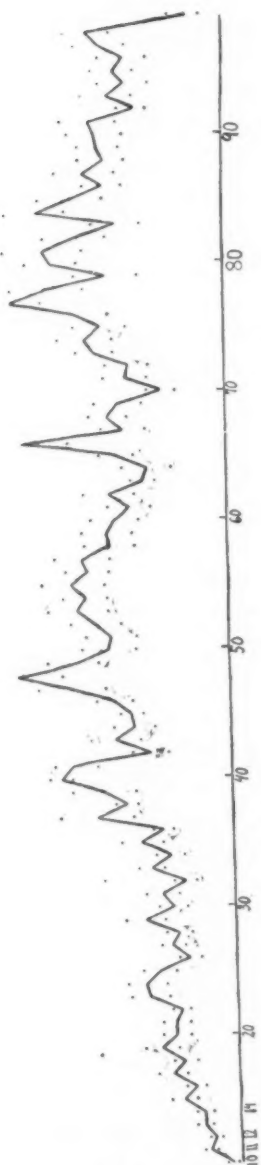


FIG. 1.

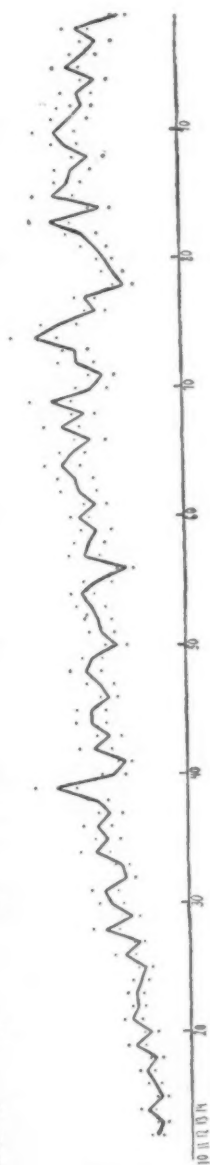


FIG. 2.

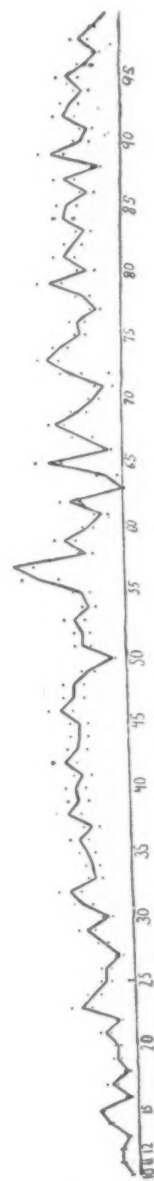


FIG. 3.

'limits of error.' The 'limits' are here 1.0 and 0.7, and the probable difference 1.2 sq. cm. The actual difference is 2.6 times the probable. In this whole series we find 6 other instances in which the actual difference is over 2 times the probable. From the probability integral we find that, in the long run, 46 actual differences to the thousand would exceed twice the probable. The question is, therefore, what is the probability of finding as many as 7 such differences in a series of 90? This is a form of the familiar problem in probabilities: to find the chances that an event whose probability is p shall occur at least r times out of a possible n . The solution depends on an application of the binomial theorem, and may be evaluated by means of logarithms. In the present case, the value found is .1209 or about $\frac{1}{8}$.

Instead of vaguely saying that the large jumps seen in the curves may be due to chance, we are now able to state that the odds are 7 to 1 against this view, and 7 to 1 in favor of the view that the large jumps, or some of them, are significant of inequality in the person's power to estimate nearly equal areas. These odds are of course not very heavy from the standpoint of scientific criticism. But they are fortified by finding, as we do, the same general balance of probability in all of the series examined. In one other series, the number of large differences is small, and the probability is as large as .2938 that they are due to mere chance. In three other series, this probability is very small, measuring .0025, .0025, .0028, or about $\frac{1}{400}$. Finally, in the series corresponding to Fig. 3, there are a large number of actual differences which far exceed the probable. (The errors are small, and consequently the probable differences are small.) There are 31 that exceed twice the probable difference, and of these 9 exceed 3.5 times the probable difference. The probability of finding even these 9 is so small that six-place logarithms cannot determine it exactly, but it is less than .000001.

In four cases, then, out of six examined, it is altogether inadmissible to attribute the differences to chance, while in the other two the odds are against doing so. The probability that the differences in *all* the series are due to chance is of course

multiply small. The differences are therefore not chance, but significant; the ability to judge one magnitude is sometimes demonstrably better than the ability to judge the next magnitude; one function is better developed than its neighbor. The functions of judging nearly equal magnitudes are, sometimes at least, largely separate and independent. A high degree of ability in one sometimes coexists with a low degree of ability in the others.

THE PROBLEM OF A 'LOGIC OF THE EMOTIONS' AND AFFECTIVE MEMORY.¹ I.

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After the philosophical and psychological turn to the conception of the primacy of volition in the mental life, the problem of a 'logique émotionnelle,' of a special immanent process by means of which affective values become generalized and objectified, could not long remain in abeyance. The intellectualistic point of view conceives the affective life, so to speak, as an epiphenomenon, a merely functional expression of the relation of ideas to ideas. All continuity of the mental life resolves itself into associational or logical relations of ideas. On the other hand, the biological point of view derives continuity from the motor relation of the organism to environment.

Now it is just this problem of continuity in the sphere of affective life that must come to the front as soon as the concrete mental sciences, ethics, æsthetics, etc., turn from intellectualistic to volitional and emotional points of view. There is no doubt that these sciences have been more and more setting themselves the task of analyzing and describing objectified and projected values, which spring from the volitional and affective life, and that the results of this study have presupposed mental processes which until recently have had very inadequate basis in traditional psychological theory. This continuity in the sphere of values, semi-logical in its nature, presupposes a generalizing and abstracting process upon affective values as such, giving rise to the so-called emotional abstracts, sentiments and moods, relatively independent of the particularizing ideal elements and capable of subsuming particular emotions under them.

¹ A paper read in part before the American Psychological Association, Dec., 1900.

It is often denied at the outset of a discussion of this nature that psychology can have anything to do with the description of meanings or values. These attach themselves to complex states of consciousness, and the analysis of these states into their elements fails to find anything in the elements explanatory of the values of the complexes. Values are to be appreciated, not described. A certain delightful mystical faith in values cannot be denied this point of view, and it has the advantage of being negative and of avoiding the errors of the positivistic school. A fundamental fallacy, however, it cannot escape, in that it overlooks the fact that in objective experience there exist worth projections whose meaning is so clearly determined as to be universally used as descriptive categories for mental states and to be themselves capable of description.¹ If this agnosticism means that the ultimate significance of values lies outside psychological method, this can lead to no dispute. Values are, however, once and for all in consciousness and, as such, have a right to an adequate psychology. It is curious how this agnosticism fails to see the serious implications of such reasoning for other spheres of psychology. Are not the objective conceptual values of cognition, spacial, temporal, the starting points of the psychology of space and time? Is not the problem just this—to understand the genesis of that which is now ordered and objectified meaning? It is more than probable that the temporary reaction against bringing the objective values of the æsthetic and ethical consciousness into the sphere of psychological analysis has been justified merely by the present state of psychology, but cannot be of fundamental and final significance.

The one assumption that underlies recent developments in ethics and æsthetics is this—that the evolution of sentiment, which all development of values involves, consists in a specialization of emotional dispositions, making them relatively inde-

¹ The antithesis between appreciation and description is one which cannot be pressed far without involving contradiction. There is a moment of value in all descriptions. Münsterburg, with all his emphasis on this distinction, in his '*Der Ursprung der Sittlichkeit*,' opposes the Wundtian ethics by insisting upon the sentiment of obligation as a psychological ultimate and the test of all moral consciousness!

pendent of the particular stimuli which call them out. These emotional dispositions having become fixed in consciousness by linguistic usage become relatively permanent values, capable of subsuming under them other dispositions and in turn being subsumed by judgment under higher values. A historical illustration of the actual specification of such an emotional value is the differentiation of the sentiment of sublimity in Neo-Platonic times. Doubtless those various emotions, later described as the mathematical and dynamical sublime, existed as separate emotions before the time of Longinus, as of course art itself proves; but this grouping under a new term, this specification within the general sentiment of the beautiful, is the result of a process of abstraction exercised upon the emotional reactions themselves by raising into clear consciousness a distinct qualitative element in emotion, a qualitative element the nature of which shall be considered later. The point of interest here is that a study of Longinus shows that he recognized clearly the varied ideal content that brings forth the reaction which we call the sentiment of sublimity. Such a specification of emotional values may be called a generalization of emotions. It results in a sentiment or emotional abstract capable of subsuming under it the particular emotions of which it is the abstract.

We have now to consider (1) the grounds for a theory of emotional abstracts, (2) the nature of the quality of an emotion that is brought to consciousness in the process of abstraction, and (3) the nature of subsumption of particular emotions under sentiments and moods.

But before entering upon this problem it is well to recognize the fact that there is already in existence a theory which, if adequate, would afford a much simpler explanation of the continuity of the affective life. Ideas, we are told, are generalized and abstracted, and as this process proceeds the intensity of the hedonic element and of the organic sensations decreases, but the feelings connected with general ideas are the same as those associated with the particulars. The assumption is that all affective states are reducible to varying intensities of pleasure and pain and varying qualities of organic sensations. Thus, for example, an organic reaction, can be con-

ceived as successively associated with various ideas and even with general concepts. The difficulty with this association theory is its inability to account for certain characteristic phenomena in ethics and æsthetics. The ability of consciousness to pass judgment upon emotional tendencies or moods and sentiments independently of the ideal content which calls them forth makes them intrinsic values apart from the lapsed links, causal and associational, which have connected them with ends. The point to be observed here is that in the very dropping of these links a generalizing process has taken place, making the emotional disposition relatively independent of ideal content. In the second place, in the phenomena of transference and expansion of feeling, of sentiments and moods—or, on our theory, emotional abstracts—over cognate and related material, we find facts which point to a relative independence of the feeling or emotional abstract thus expanded. The expansion or extension of the feeling abstract may be discussed under two heads: (1) the æsthetic extension of a sentiment over particular feelings and emotions, and (2) the ethical phenomena of emotional sanction.

Both Meinong and Ehrenfels¹ have pointed out with considerable subtlety that the phenomena of transference of sentiment and its expansion cannot be explained by mere association of ideas. There is an element of judgment in the process which makes it of the nature of a subsumption. In recent æsthetic discussion, in place of the older conception of ideal unity, emotional unity has been substituted, in many cases without a clear understanding of what is thereby assumed.² Ribot has seen that such a conception implies the existence of emotional abstracts which subsume varied feelings and emotions under them. Now the emotional unity of a work of art consists in the fact that a given mood or sentiment can be maintained throughout, containing in it the varied particular emotional tendencies which are started by the particular content which is subsumed. The

¹ Meinong: 'Psycholog. ethische Untersuchungen zur Werth-theorie,' p. 61. Ehrenfels: *Vierteljahrschr. f. wiss. Philos.*, 1893, p. 216; also, 'System der Werththeorie,' Bd. I., 122, Bd. II., 154, 156.

² Compare the writer's criticism of Hirn's 'Origins of Art,' in this same number.

technical conditions of this expansion will be brought out in a second paper on the æsthetic attention. It is sufficient to point out here that the great work of art is that which contains the most varied emotional tendencies, all subsumed under the one emotional abstract.

A striking illustration of this is that exquisite mood poem of Tennyson's, 'The Lotos-Eaters.' All the technique of imagery and rhythm is expended not only to arouse the gray mood of forgetfulness and indifference, but to carry it on and on, intensified and fastened, until it becomes the mood of the very gods themselves through which they see the world. That world includes many things which do not fit this mood; yet, like gods,

"They smile in secret, looking over wasted lands,
Blight and famine, plague and earthquake, roaring deeps and fiery sands,
Clanging fights, and flaming towns, and sinking ships and praying hands."

But although these rapid pictures suggest incipient emotional responses of another sort than the dominant mood, they succeed each other so rapidly that it is

"Like a tale of little meaning, though the words are strong."

The general mood may become so strong that it will spread over all particular emotional tendencies, provided the technique of expression is such as not to allow these particular motor tendencies to get above a certain strength. In this case the technique consists in the rapid piling up of the pictures, thus preventing the particular emotional suggestions from getting in their full motor value. Experiments with people in the reading of this poem have led to the conclusion that if the mood of the poem is fully appreciated before this passage comes, it may be subsumed under the dominant mood. Otherwise the contrast is too great and the unity of the poem seems to be broken. So much for the passive appreciation of the relation of mood to particular emotions. On the side of active, creative expansion of a sentiment or mood over particulars, we have the interesting account by Poe of his construction of 'The Raven.' He tells us¹ that his starting point was the purpose to express the

¹ Poe: 'The Philosophy of Composition'; also Paulhan: 'L'Invention,' p. 81; also Ribot: 'L'Imagination Créatrice,' where Poe is classed as of the *diffuente* type of imagination, where the unity is the emotional abstract.

mood of melancholy. For this mood he found the characteristic refrain 'Nevermore.' His art then consisted in finding particular images, such as the raven, the locality of the poem, etc., which had emotional tones which could be subsumed under this mood. And then he did not scruple deliberately to tone down all ideas and images whose emotional intensity was too great for the dominant mood.

This expansive tendency of the generalized forms of feeling, mood and sentiment is shown also in the facts of ethical emotional sanction. The sentiments of shame, tragical elevation, and the moral moods and sentiments in general expand themselves over the particulars of affective experience, and it is this subsumption which sanctions the particular experience.¹ On its affective side ethical judgment consists in the subsumption of particular feelings under general forms of emotionalism which have become, through the usages of speech, well-defined objective values. Thus, Guyau² has formulated the end of the ethical life as a maximum extension of sentiment values consistent with intension of meaning. By this formula he is able to bring the ethical life into relation with the æsthetic and subsume them both under the common principle of maximum expansion and intensity of life. Out of this functional view of the moral end, which of course presupposes the doctrine of emotional abstracts and likewise a judgmental relation between these general sentiments and particular emotional tendencies, arises a distinction of importance in the present discussion, the distinction which in the logical sphere is made between the empirical and logical extension of a concept.

There is first of all, then, a purely empirical extension of an emotional abstract; that is, its 'Expansionstendenz' is limited by purely empirical conditions. The first of these is, as Ehrenfels has pointed out,³ that a mood or sentiment tends to extend itself over all content of consciousness associated in time with the original stimulus of the mood. Upon this fact the first and most superficial unity of a work of art and of ethical judg-

¹ Ehrenfels: *System der Werththeorie*, Bd. II., 5, 154.

² Guyau: *'Equisse d'une Morale, etc.'*, Livre II., Ch. I.

³ *System der Werththeorie*, Bd. II., pp. 147, 148.

ments rests. To an uncultivated taste a strong sentiment, however crude, may envelope an entire poem, notwithstanding glaring emotional contradictions and contrasts which are patent to the more critical mind. Of the same nature are many of the illusions of ethical judgment. Beside this merely empirical expansion of feeling there is one which we may, by analogy, call the logical extension. The nature of this 'logical' extension we can discover only by making clear wherein the common element in both the particular and the abstract emotion exists. For such an extension of the emotional abstract over particulars must be of the nature of subsumption, and the relationship between the two must be judgmental in character. There must therefore be some 'equivalence of value' between two affective states for them to be related in this way. Now since all extension of an affective state which rests upon judgments of equivalences of value, as distinct from the mere empirical expansion, is conceived as æsthetic in its nature, whether it is found in the ethical or artistic experience, we shall call it 'æsthetic' instead of logical extension.

This tendency of simple feeling or emotion to pass over into sentiment and mood, or, on the other hand, for sentiment or mood to resolve itself into a particular emotion, is familiar to all students of the affective life, but it has assumed importance only where the question has been asked wherein lies emotional consistency of a work of art or affective continuity and consistency in the ethical life. To determine the criteria of the relation of these total and partial feelings then becomes important. Titchener, in his chapter on sentiment, tells us that "it is natural that sentiment, which is developed out of emotion, and characteristic of a higher stage of mental differentiation, should readily slip back into emotion." But his explanation of the phenomenon is inadequate, for the reason that just this development of sentiment out of emotion is left undescribed. We pass from the particular emotions of the poem or the novel to the æsthetic sentiments of harmony which are the affective attributes of judgment, but upon what this relationship between the emotion and the sentiment rests is not disclosed.

In this connection arises a question which becomes of still

more importance later—whether there is this direct dependence of affective state upon affective state which the doctrine of abstract and particular and of emotional unity and continuity presupposes? In recent psychological discussion the question has been asked: what contents of consciousness can become the ‘presuppositions’ of feeling? Presentations, sensation and idea, are of course conditions of feeling states. Wundt has made us familiar with the feelings brought out by concept and judgment, and Meinong with *Urtheilsgefühle*.

But may not feelings themselves be the ‘presuppositions’ of other affective states? This relation, of so much importance for an emotional theory of æsthetics and for a general theory of value, has been answered by a number of investigators in the affirmative. Meinong and Ehrenfels, Pillon and Witasek, all recognize its existence.¹ It seems probable that feeling may be the ‘presupposition’ of feeling in two ways: Memories of past affective states may call up new affective reactions, as when, in the case cited by Pillon, Saint Preux’s memories of the past are followed by new feelings of the present, of which they are the ground and presupposition. Or, in the second place, feeling may be the presupposition of feeling in the sense that in order that certain feelings may be experienced at all (this is particularly true in the case of represented feelings in art) there must be, as presupposition, a dominant mood, in harmony with it. Or perhaps it would be better to say that the affective state is part presupposition with the images of the emotion which shall be evoked. It is this second form of relation— affective states as the condition of affective states—that becomes so important in æsthetic effect. The emotional effect of those few lines in ‘The Lotos Eaters,’ descriptive of burning towns, sinking ships, etc., is entirely determined by the mood which precedes.

We may observe in concluding this point in the discussion, what is already evident to the reader, namely, that the whole question of presentation and representation to consciousness of the

¹ Ehrenfels, Bd. I., p. 207; Witasek, ‘Zur psychologischen Analyse der ästhetischen Einfühlung,’ *Zeitschrift für Psych. und Physiol.*, Bd. 25; Pillon and Mauxion, *Revue Philosophique*, February, 1901.

affective state as such, therefore of affective memory, is already anticipated. No one should hesitate to admit that mere pleasantness or unpleasantness as such cannot be the presupposition of mere pleasantness and unpleasantness, any more than can these hedonic elements, abstracted from the *fusions* that alone have meaning for consciousness, *i. e.*, emotions, sentiments and moods, be remembered. The question is simply this: have these fusions such unity as to make them capable of presentation and representation to consciousness? If so, they follow the laws of dependence of representatives upon each other.

Now if we examine the four orders of affective experience, simple feeling, emotion, sentiment and mood, we notice that, while for purposes of abstract analysis the simple feeling may be distinguished from the other forms, the ultimate of actual experience is the simple emotion. Although a sensation may conceivably be affectively toned without leading to any emotional reaction, in reality that affective tone has no meaning until it passes into some tendency to motor reaction. The principle that the ultimate of meaning is the motor idea, is strikingly brought out in its philosophical significance by Professor Royce in 'The World and the Individual.' It is equally true that no adequate theory of affective complexes or abstracts can be built up out of the simple elements of pleasure and pain and ideas. It is only when the selective processes of generalization take hold of ultimates that have conscious meaning, namely, motor tendencies in their relation to volition, that complexes of higher meaning may be developed. The ultimate of meaning, then, being the particular emotional tendency, the affective states of a higher order, in case they shall relate themselves to the particular emotion as general to particular by subsumption, must be conceived as developed from these by processes of generalization. The characteristic of mixed feelings, as Höffding calls the higher complexes, is that they make up a total feeling, a fusion, in which the parts are indistinguishable, and yet one out of which the particular is redeemable again. Whether we call this process, by which out of simple emotions, sentiments and moods are developed, *fusion* (Höffding, Elsenhaus) or *generalization* (Ribot), the conditions which make

possible the relations between the total and the partial feeling are the same.

First, then, let us consider what may be called the negative conditions of this relation. By sentiment is understood the total feeling connected (*a*) with any word which stands for a generalization of particular experiences; these are the concept feelings, such as connect themselves with words like *love, death, dreams*, etc.; or (*b*) with well-differentiated groups of sensations or ideas which remain relatively permanent, such as the sentiments connected with home, town, places and persons in general. By mood, on the other hand, is meant the total feeling arising (*a*) from the fusion of separate organic sensations into vital feelings, such as melancholy, depression, buoyancy, etc. Or (*b*), secondly, a generic phase of emotionalism made up of vital feelings together with generic emotional tendencies, such as the moods of reverence, playfulness, etc., according to what the nature of the vital feelings may be—for instance, a melancholy playfulness, a sombre or cheerful reverence. In all moods of this second order the element of judgment enters and the sentiment is modified by the mood. Finally, we call those affective states moods which gather about a group of sensational or ideal elements, but where the vital feeling is predominant, as the mood of a place or landscape. Upon examination of all these phases we find in their structure conditions which make possible, negatively at least, relations of subsumption. In the first place, in all these generic phases of emotionalism is noticeable a dampening of the intensity of the hedonic and sensational components. For it is only when a particular set of organic sensations or definite emotion becomes dominant that a mood can become intense. In addition to this, it seems probable that in the mood, at least, the number of the component or organic sensations increases the fusion and lowers the intensity of the component parts. In the concept feelings the hedonic element is, as has often been observed, of less intensity than that connected with the particular images which are the presuppositions of the particular feelings, and the organic sensations qualitatively less definite. The technique of 'The Lotos Eaters' illustrates the bearing of these facts upon the question of subsumption. Such imagery

as is employed in this poem is always in vague, general terms, so that the affective resonance may be dampened and not overcome with particular emotions, the languor of the general mood developed by the rhythm and the careful use of vowel sounds. "Here are cool mosses deep"—"the ivies creep"—"the long leaved flowers weep."

The same criteria of dampened intensity and qualitative indefiniteness are found to apply in greater degree to the other forms of sentiment and mood. The sentiments formed about places, or the ethical and religious sentiments, are fusions of many particular emotional reactions (wherein this likeness consists we shall see later), which cause the particulars of ideas and organic sensations to become indefinite and the hedonic intensities of the particular emotions to sink to a common level.¹

Upon what positive element, then, does the selective attention act in the generalization of generic phases of emotionalism from particular emotional reactions? The most common notion is that the ideal element alone distinguishes the sentiment from simple feeling, while the mood is different from the emotion merely in the fact that it consists of a greater summation of organic sensations and in the length of time it lasts (Titchener). These distinctions do not, however, do justice to the objective nature of these projections of affective values. The lapsed links, ideas, hedonic intensities and organic sensations which the psychologist in his reconstruction of these emotional states supplies, *are* lapsed links—have escaped attention—are no longer constituent elements of the projected value, the objectivity and independence of which make its extension over particular feelings possible. There is a constant element in these emotional abstracts which is to be found in none of the elements into which psychology analyzes the emotional complex. The contention of

¹ These same criteria of (a) weakened intensity and (b) qualitative indefiniteness have been developed by Elsenhaus in an article: 'Ueber Verallgemeinerung der Gefühle,' *Zeitschrift für Psych. und Phys.*, Bd. 24, Hefte 3 and 4. He also brings out the point that the representation of a general feeling by a particular differs from the representation of a concept by a particular in the fact that as soon as the general passes over into the particular or partial feeling, the latter tends to expand over the entire affective life, qualitatively determining it.

the agnostic, at this point, to which we referred at the beginning, is in so far right, that the universals of affective meaning can never be explained by analysis into the abstract elements, sensation and simple feeling. The retrospective psychologist centers his artificial attention upon the elements into which the psychosis has been analyzed; but in pursuance of the 'acquisition of meaning' the selective attention must have been directed upon just those values of the psychosis which the abstraction of the psychologist ignores. We repeat the question, then. Upon what positive element in the particular emotional reactions does the selective attention fasten in its generalizing process? What positive element remains constant in sentiments and moods, making it possible to subsume under them particular emotions in such a way as to give æsthetic truth?

The hypothesis of qualitative elements in feeling, different from those of pleasure and pain, is one that is constantly coming forward in those quarters where the starting point for a psychology of the affective life is the value-resultants as they are found in the projections of the ethical and æsthetic judgments. Thus, in addition to the Austrian school of psychology, Krüger, in a monograph entitled '*Der Begriff des absolut Werthvollen als Grundbegriff der Moralphilosophie*,' insists upon the existence of feelings of value as such, different from pleasure and pain, and seeks to point out that feelings are valued not only according to their intensity and duration, but also for their breadth and depth in the personality. The criticism which such conceptions call forth, certainly presented in one of its most searching forms in Külpe's '*Introduction to Psychology*,' owes its force largely to the notion that the starting point for constructions of a higher order in the affective life is a simple feeling. If every feeling attached to a sensation or idea has its peculiar affective quality we are no doubt burdened with a superfluity of qualitative richness, but, just as in the intellectual life generalizations and abstractions start with a percept or idea and not with a sensation to develop the relationships which give meaning to thought, so whatever meaning may develop in the affective life through generalization must start with the ultimate of meaning, the emotion.

Now any emotion that I may call up, waiving for the time being the question whether an emotion can be recalled (the problem of emotional memory), is a generic image. When I try to realize an emotion, say fear, I find a succession of motor tendencies and images. I start back, shrink into myself, close my eyes, in short, my representation of fear is a series of images, of motor tendencies that I have experienced at different times. It is generic. If I analyze further and try to find what images acted as stimuli for these different motor tendencies, I find them gradually trooping into consciousness and associating themselves with these various motor tendencies. In this case, with the starting back comes the image of the locomotive which I once but barely escaped. The shrinking into myself is vividly connected with a particular experience of supposed social disgrace, the closing of the eyes with a threatening face. Now the point here is that these generic motor tendencies appear first, and they are then associated with visual images but are themselves revivable independently of the visual images. These motor tendencies, then, are all parts of the generic emotion fear; but this is not the extent of the analysis. What makes these motor tendencies parts of one generic emotion is the dynamic element they have in common. However these reactions may differ in the quality of their organic sensations, there is a dynamic resonance which may well be called the dynamic constant that makes them generic. By the *dynamic constant* of a generic phase of emotionalism is meant nothing more than a relatively permanent system of intensities and of temporal and rhythmic relationships among the organic sensations of an emotional reaction. If one examines the peculiar qualitative distinctness of an emotional state, it will be found invariably capable of expression only in terms of resonance of the whole organism. To describe these qualities of the emotional state we must invariably have recourse to some figure from the outer world or some system of time or intensity values which shall stand as symbols for the inner system. Thus Mauxion, in the article already referred to, has pointed out that the increase of brightness intensities in a sunrise and the corresponding decrease in a sunset correspond to definite affective resonances.

Now it is not at all inconceivable that when we speak of an angry puff of an engine, or of a long drawn sigh, we are putting into figurative expression the real characteristics of inner emotional reactions; and it is also conceivable that, if the unitary quality of an affective state consists in a system of time and intensity of relations of the component organic sensations, the characteristic of the resonance will remain constant no matter how the absolute intensity and the qualitative definiteness of the mass of sensations may vary. Just as in the fusion of tones into chords the unitariness of the fusion is affected only by the relative intensities of the components and not by the absolute, it is quite conceivable that the unitary nature of a generic emotional state consists not in the quality of the organic sensations but in a system of relative intensities and time values.

The advantages of such a conception of the constant element in generic affective states are obvious. It enables us to see, not only how the expression of emotion in objective forms is possible, but also how this very expression of relationship, especially in rhythm and light and shade values, has the effect of bringing to consciousness the most intimate quality of affective states, segregating them by processes of abstraction from the qualitative ideas of the stimuli and the organic sensations. It enables us to understand how words, whose emotional values are ultimately in the emotional tendencies involved in their pronunciation, may express abstract emotional states even when the processes of generalization have lowered the intensities of the organic sensations and made their qualities indefinite. Finally, we would then be able to understand how representation of an emotion itself is possible, for this dynamic constant character of an organic resonance could be represented without the hedonic being prominent and even through varying organic sensation content.

In such a notion that the essence of an emotional state is a *dynamic constant* there is nothing inconsistent either with experience or with recognized psychological conceptions. For, in the first place, the body of an affective state being made up of organic sensations corresponding to definite organic tendencies, if our recognition of an affective state as such depends upon the

definiteness of the component sensation we ought to recognize a particular emotion much more easily than those sentiments and moods in which the qualitative elements are less definite. This, as we shall see in our later study of affective recognition, is not necessarily true. Again, the organic sensations of an emotional state, as has been pointed out, vary in the quality of their components. There are different fears, rather than a single reaction of fear. Any one of these particular fear reactions may stand for the generic state of fear, as a particular may stand for a general concept, and yet it represents the generic emotion only in virtue of something constant which makes them all capable of assimilation along the same general paths of motor habit.

As to the concept itself, it is already familiar in other spheres of psychological analysis. Under the term '*Gestalt-Qualitäten*' is coming to be understood such systems of relations as remain permanent amid changes of content. Thus, for instance, a melody may remain constant for recognition though expressed in different keys, in different tone series. This unity is, however, so permanent in all its expressions and in its further use in higher tone creations, that it may be conceived of as a complex which is fundamental in its character, '*ein fundierter Inhalt*.' Among these '*Complexionen*,' as the writers of this school call them (Ehrenfels, Witasek), Ehrenfels suggests that the fundamental æsthetic reactions, the sentiments of the beautiful, the sublime and the comic, may be placed.

In any case we are now in a position to outline a possible theory of emotional abstraction and generalization. We have already seen that those affective states which bear the marks of abstraction—concept feelings, sentiments and moods—are characterized in general by lower hedonic intensity and by qualitative indefiniteness, and yet their unitary quality stands out strongly. This is, however, just what would be expected if the process of abstraction consists of the bringing into prominence by selective attention of a fundamental quality other than the varying elements of hedonic intensity and qualities of sensations. The first stage of this generalizing process is then the generic emotion itself, which, as we have seen, should be properly distin-

guished from the single emotional impulse, it being made up of a number of motor tendencies manifesting themselves in consciousness in various organic sensations, qualitatively different, but each group having the common dynamic constant. The genetic theory that the body of an emotion is made up of survivals of particular impulsive movements conceives our experience of an emotion as generic, as a fusion of organic states coming from varied particular reactions, and Professor Baldwin has expressed this same idea by the theory that the development of sentiment is a result of the assimilation of new content to old motor tracts, thus making of the emotion a generic tendency rather than, as the association theory conceives it, a single element associated by habit with various ideas. It is, however, the very nature of such a process of assimilation to bring out the constant dynamic resonance of an emotion, and it is as such that we cognize it as a definite affective experience.

Still more generic phases of emotionalism may be conceived of as developing through the same processes of assimilation. Like the sentiment of sublimity, the historical origin of which was used as an illustration, the great sentiments of the ethical, religious and æsthetic experience—as for instance faith, reverence, playfulness, the comic—may be looked upon as complexes of a higher order, as assimilations of varying emotional tendencies on the basis of their dynamic constancy.

They represent generic attitudes of emotional response to which new emotions of the same dynamic constant may be assimilated. This enables us to understand the development of sentiments, their expansion, ever including finer *nuances* of particular emotions, and at the same time their relative permanency. Undoubtedly the entire growth of sentiment, as Guyau has described it in his beautiful chapter on the 'Evolution des Sentiments,' has consisted in the expansion of the sentiment over more and more ideal content; and in this expansive tendency of the sentiments lies the possibility of subsuming, as Guyau hopes, the scientific ideas of his time. But just this expansion, as we have it in art (especially poetry), consists in the development of new concept feelings, those complexes of partial emotional tendencies, of such character that they may assimilated to the

old sentiments. This leads us to a final problem, which can be merely touched upon in the limits of this paper, a problem however, for which the present discussion is largely introductory—namely, what is an emotional value; what relation has this concept of the 'dynamic constant' to emotional values, and how are emotional values related in the ethical and æsthetic consciousness? An examination of the more objective and permanent sentiments and moods discloses the fact that their description as well as their valuation—their meaning, in other words—is invariably in terms of their relation to volitional efficiency. Viewed objectively, they have value as dispositional presuppositions of overt acts of impulse and will. As extrinsic values, they are conceived as the causal presupposition of judgments and acts. As intrinsic values, however, viewed inwardly, we have described them as 'dynamic constants,' and undoubtedly their meaning and value lies in their dynamical relation to volition, to inner activity, in their efficiency for volition.

On the theory that volition is primary in the mental life, which underlies so much of modern psychology, and, secondly, on the theory that the will manifests itself only indirectly in the affective life—in the complexes of sensations and affections which constitute emotion, sentiment and mood—we may see how some such concept as that of the 'dynamic constant' in affective states and the theory of emotional abstracts as here understood would afford a basis in psychology for a doctrine of values. It may be possible, then, to say with Krüger that the value of feelings is measured not only by their hedonic intensity but by their breadth and depth in the personality. But the relation of the extension to the intention of a sentiment and the general question of the relation of sentiment, æsthetic and ethical, to volition, are problems that must be left for another occasion.

DISCUSSION AND REPORTS.

MULTIPLE AFTER-IMAGES.

In the July issue of the *PSYCHOLOGICAL REVIEW* for 1899, under the caption of *Notes on After-Images*, I called attention to the phenomenon of a different after-image for each eye simultaneously, the proof resting on the fact that their colors were different, the one being positive while the other was negative, and vice versa. Now I wish to record another interesting observation in relation to images. The matter started as an experience and became an experiment.

On a recent morning I noticed a bright image on the floor of my bedroom and supposed it was due to the sun at which I had just looked. I tested the matter by looking again at the sun. I projected the image on the wall of my room and was surprised to find not a single image but three, side by side, in the negative stage. They soon became two and then merged into one. All the more curious, I again viewed the sun and returning my eyes to the wall discovered five images, two of which were positive and three negative. Keeping at this process I secured as many as fifteen images at one time, some of which were positive and some negative, and all clear and distinct. Then I turned my attention to the sun. I found a new phase, namely, that instead of being eclipsed by the sun, the after-images persisted while I looked at that orb and appeared in the field of vision all about it. The number increased meanwhile, by what means I did not know, and three different colors appeared among them. These triple colors, however, came after I had projected a group of images upon the wall of my room. Returning my eyes to the field of the sun I discovered an intense lemon color, besides a positive and negative color of the sun itself. On the succeeding morning I found the images could be multiplied indefinitely and that at least five distinct colors could be secured simultaneously.

Such are the data. How are they to be explained?

First, the number of images is due to the great intensity of the stimulus, together with the orientation of the eyes. Images under a bright sun form instantly. Then the rotation of the eyes brings new

regions of the retina under the direct assault of the stimulus and each new spot impressed gives out an image.

Second, as to the visibility of the images under the full light of the sun. At first I thought this was due to the indirect rays of light being sufficient to cause them to persist. Now I believe it is due to the orienting of the eyes, which passes the stimulated regions again under the direct rays of the stimulus and so renews the stimulus. This seems true because by fixating the sun for a minute the surrounding images fade away.

Third, the multiplicity of colors evidently comes from color mixing upon the normal principles of that process as conducted in the laboratory. For instance, the deep lemon color I found to be compounded from the images projected on my drab-colored wall carried over to the blue field near the sun. The other different colored images were made from combination of images in different stages of passage from positive to negative. The possibility of different hues is very great.

One interesting feature of the first experiment I have not been able to reproduce. Upon getting a great number of negative images of the sun projected into the field about it, then slowly turning the eyes upward, the images ran together in a ball and then taking the shape of a balloon floated slowly upward.

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(Signed) EDWARD FRANKLIN BUCHNER,
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PSYCHOLOGICAL LITERATURE.

Grundzüge der Psychologie. Von HUGO MÜNSTERBERG. Band I., Allgemeiner Theil: Die Principien der Psychologie. Leipzig, 1900. Pp. xii + 565.

Professor Münsterberg is a conspicuous figure among the writers and thinkers of both America and Europe, and whatever differences of view he may encounter he is sure of attention and respect. His latest work, moreover, *Grundzüge der Psychologie*, to judge only from the first volume recently published, would of itself have secured to him this enviable position, if his special work in empirical psychology, and among other things his more popular volume, *Psychology and Life*, so widely read and so much discussed, had not already compassed it. In Professor Münsterberg's history, however, as well as in the history of the science which he has represented, the *Grundzüge* is a peculiarly noteworthy book. Thus it is nothing more nor less than a systematic presentation of its author's rich experience in the field of psychology. Perhaps, at least with reference to this first volume, since some would separate reflection and experience, one should rather say that it is a presentation of his reflections on a rich experience in psychology, the more direct and special treatment of the matter of psychology itself being reserved for the volume that is yet to come; but in any case, whatever one ought to say, it is plain from this first volume that Professor Münsterberg has not been a passive observer, that for him the unexamined life of psychology has not seemed worth living.

And besides having this sense of the need of self-examination, so commonly felt among the sciences of to-day, psychology is to be made timely in another way. Thus it would exact from its followers, or to say the least it has plainly exacted from Professor Münsterberg, the 'strenuous life.' I say this with all due apologies for my profanity. Throughout the present book the 'I want to fight' of the Preface of *Psychology and Life* is distinctly to be heard. Everywhere back of the words one feels in an unusual degree the presence of a strong will. The style is vigorous and, while I myself could never sympathize with those who have chosen to object to Professor Münsterberg's English, the use of the German has undoubtedly enabled a freer and so a more

effective expression. Moreover, with regard to the use of German, the report that the author will not consent to the translation of his work is probably true and may be interpreted as showing how seriously he takes himself. The very life of psychology is at stake, and with more than mere fancy one must say that the German has come to defend what more than any other the German brought into being. In short, from start to finish, this book is a book with a moral purpose; it is no lifeless contribution to the world's stock of knowledge or mere speculative doctrine.

Hence, although perhaps less openly here than in *Psychology and Life*, Professor Münsterberg shows himself to be always thinking, not only of old theories to be criticised and possibly refuted or of new theories to be formulated, but also of men or classes of men to be brought into line and made to go in their right ways, in the different ways of the special sciences or in the way of the 'real life.' It is perhaps only the most external or circumstantial evidence of this that is afforded by the careful and valuable bibliographies given with the different chapters, but such a classification of the books and papers bearing on matters psychological, when taken in connection with the moral purpose and personal spirit of the book, is certainly not without significance. Professor Münsterberg would be a personal leader as well as a scientist or a thinker, and his book, I venture to say, will appeal to its readers from this standpoint, winning disciples or stirring up antagonists in a way that few books are privileged to do. This, of course, is desirable and very refreshing, and it is especially pertinent in this review because, in a way to be pointed out in a later paragraph, it is itself a comment on one of the fundamental contentions of the author.¹

Now, as has been said, the present volume is an attempt to make psychology self-conscious. Psychology must know what it really is and particularly what it is not, and all the other sciences as well as all the unscientific relations of life need to be informed on these points too. Definition of the field of psychology was, it is true, the purpose of *Psychology and Life*. Here, however, we have no miscellaneous essays for widely different classes of readers, but instead, a uniform treatment, that would be exhaustive and that is addressed especially to

¹ At the 1900 meeting of the American Psychological Association, as well as at that of the Western Philosophical Society, Professor Münsterberg's influence was very obvious. At the former, for example, two of the papers read were, one in part and the other in whole, open criticisms of his standpoint, as presented chiefly in *Psychology and Life*, not to mention certain magazine articles, and others seemed to have that standpoint in mind.

students and teachers in science and philosophy, of the exact province of psychology. And what is the result? The result is a perfectly free object, in the sense of a wholly distinct object for psychology; an object that is wholly divorced in the first place from the will or feeling of the subject—the 'real' subject, of course, not the empirical subject—and in the second place from the proper objects of all the other special sciences as well as from what are not properly objective sciences but historical or normative sciences. "Dieses Buch," says Professor Münsterberg (p. vi), "will der empirischen Psychologie vollkommenste Freiheit sichern und doch das unbegrenzte Recht des Idealismus dathun."

With reference to the latter half of this program it is hardly necessary to say that the cause of a moral Idealism is quite as near to Professor Münsterberg's heart as that of science in general or even of psychology in particular, and what accordingly he proposes is a conciliation, 'auf neuem Weg,' of these two interests. The way, moreover, whether novel or not, in which this conciliation is to be effected is announced thus: "Der Idealismus kann nur dann sein Recht behaupten, wenn er in seinem eigenen Umkreis Platz für eine rücksichtslose konsequente empirische Wissenschaft hat." Science, in short, is to be an act of the will; but in the interests of freedom, of an absolutely free object, it must be wholly innocent of any relation to the will, it must be only an *external* act of the will, will and its acts being separable. This certainly involves a difficulty, but for the moment we will waive it.

The objectivity of psychology is thus presented (p. 56): "Die Psychologie will eine beschreibende und erklärende Wissenschaft sein; sie muss als ihren Gegenstand also vorfindbare, in sich bestimmte und somit unabhängige Objekte verlangen und voraussetzen. Die Wirklichkeit bietet nur abhängige Objekte der Stellungnahme und Akte, die wir verstehen und würdigen; die Psychologie—und dasselbe gilt von der Physik—kann es mit der logisch primären wirklichen Erfahrung also gar nicht zu thun haben. Psychologie und Physik sind somit erst dann möglich, wenn das wirkliche Erlebnis verlassen und ein Abstraktionsprodukt gewonnen ist." The most significant part of this passage is the interjection about physics, 'und dasselbe gilt von der Physik.' When such extremes as physics and psychology are brought together there can be no doubt that the intention to keep psychology in the ranks of the objective sciences is a thoroughly serious one, and so that teachers and preachers and all other creatures of the real life are to have at least no more right to turn psychology into an art of pedagogy or

ethics or of anything else practical, into an ism—or a nostrum?—of any sort, than to abuse physics in the same way. For some reason they can hardly be said ever to abuse physics. Why, then, should they abuse psychology?

But the separation of physics and psychology is quite as important to Professor Münsterberg as the liberation of the object of each from all possible infection with reality or, say more properly, from having anything to do 'mit der logisch primären wirklichen Erfahrung.' Physics and psychology ought to have a common foe, that is to say, physics ought to be no more exempt from interference than psychology, and so to this extent they may be classed together, but within the sphere of the freely objective they are necessarily as far apart as the physical, equal to that which can be thought identical in the experiences of different subjects—"was mehreren Subjekten gemeinsam erfahrbar gedacht werden kann," and the psychical, equal to that which is possible as experience only to a single subject—"was nur einem Subjekt erfahrbar ist" (p. 72), in short, as the universal and the individual. And again, of the free object of psychology we read: "Das Psychische ist uns nunmehr das wirkliche, vom Subjekt losgelöst gedachte Objekt, nach Abzug derjenigen Faktoren, die Objekt für mehrere Subjekte sein können; das Psychische ist also gewissermassen *der Rest, der übrig bleibt, wenn das in verschiedenen Erfahrungen Identifizierbare, und somit das kausal Zusammenhängende, herausgearbeitet und abgezogen ist*" (p. 88). In other words the abstraction of the universal object, 'das identifizierbare Objekt,' is absolutely necessary to physics, in so far as physics is to get any knowledge of causality, while psychology must and, as Professor Münsterberg thinks, can very cheerfully content itself with what is left after physics has been thus richly provided for. Simply the psychical object is the individual object, being 'nur einem Subjekt erfahrbar,' and is therefore without continuity of its own; but this does not rob psychology of any genuine interest, since the discrete object of psychology gets unity, indirectly perhaps but not less effectively, through the physical to which it is parallel. Psycho-physical parallelism, although perhaps to be justified only on grounds of a theory of knowledge, is thus an indispensable presupposition of a science of psychology; and this, instead of robbing psychology of any positive value of its own, as might possibly be imagined, gives it value and interest. The psychical as psychical is certainly not less real or less interesting for being parallel to the physical. But, as regards the separation of physics and psychology, could any two things be farther apart than the universal and

the individual, or the continuous and the discrete, or 'das Zusammenhängende' and 'das Nichtzusammenhängende'? Surely not—unless extremes really do meet. And, in any case, it is not exactly good form in these days to turn anything so chilling as the dialectic on anybody! Moreover, the distinction itself is phenomenalist and only for epistemological purposes.

Of course human societies as well as human individuals have a mental life, a truly psychical life, and so have the animals; and the psychologist accordingly finds his proper object among these, and his field of study is thereby widely extended (pp. 97–103); but psychology is not on this account to be confused with sociology or physiology or biology, and above all psychology must not imagine that it has any right *by mere analogy* to read a psychical life into physical bodies in general, into individual atoms or into the universe as a whole. Thus: "Sowohl vom logischen wie vom psychologischen Standpunkt ist uns der Nebemensch zunächst nicht physisches oder psychisches Objekt, sondern ein aktuelles Subjekt in der Welt der gültigen Beziehungen; er ist als solches Subjekt anerkannt und nur deshalb ist sein Körper für uns mehr als ein physischer Gegenstand. Wollen wir wirklich Analogien bilden, so kann auch die anorganische Natur nur dann uns als beseelt gelten, wenn sie in der wirklichen Welt als Subjekt anerkannt wird und nun aus ihrem Subjektwesen die Doppelheit des psychischen und physischen Inhaltes abgeleitet wird" (p. 101). And further: "Das beseelte Atom als unberechtigte Verkoppelung physikalischer und psychologischer Hilfsbegriffe ist * * * die Konsequenz des Dogmas, dass die Beschreibung der physischen und psychischen Objekte alle Realität erschöpfen könnte. * * * Sobald wir aber im Auge behalten, dass alles Psychische aus der Aktualität wirklicher Subjekte stammt, so werden wir für die Psychologie das Recht zurückfordern, unabhängig von naturwissenschaftlichen Konstruktionen, nur aus dem Zusammenhange des Lebens, über das Dasein psychischer Objekte zu urtheilen, aus ihren eigenen Bedürfnissen heraus ihre Hilfsbegriffe zu bilden und einen atomisierenden Panpsychismus als unberechtigte Grenzüberschreitung der Naturwissenschaft zurückzuweisen" (pp. 102–3). In short psychology is psychology; *even* ontology or cosmology is one of the many things that psychology is not.

So much, then, for Professor Münsterberg's definition of the field of psychology, for his liberation of the psychological object, but this is only a very small portion of the whole book, which comprises in all three parts. Thus the first, bearing the general title: 'Die Auf-

gabe der Psychologie,' besides its contribution to the theory of knowledge, that has now been partly outlined here, contains chapters on 'Psychology and the Historical Sciences,' 'Psychology and the Normative Sciences'—ethics, æsthetics, logic, religion and mysticism, and 'Psychology and Life'—commerce (sozialer Verkehr), jurisprudence and education. I have ventured to translate! The second part is entitled, 'Die psychischen Objekte,' and treats consciousness, space and time, the psychical manifold. And the third part, 'Der psychische Zusammenhang,' is first given to the different theories of the unity of the self, such as (1) the doctrines of psychological *substance*, which simply make the fatal mistake of confusing psychology with metaphysics, (2) the psycho-physical hypothesis, which is of epistemological import only, (3) the theory of apperception, (4) the explanations from biology, and (5) the association-theory, and then, after this clearing of the ground, to (6) the author's action-theory. But, beyond this enumeration of the general contents and in due time some more extended reference to the action-theory, the present notice of the book must confine itself almost entirely to the matter already outlined. This in itself affords more than enough for the work of one review. In general, however, let it be said again that we have here an important study, important for what it accomplishes as well as for what it undertakes, of an epistemological justification of empirical or physiological psychology.

But, to begin with what seems to me a fundamental difficulty of Professor Münsterberg's standpoint, freedom—from the subject, das aktuelle Ich, as well as from the objects of the other sciences—may, indeed, be necessary to the psychical object as the object of a *science* of psychology, and accordingly it may be also necessary that psychology deal with what is only a transformation of reality, a logical construction; but herein lurk two inevitable dangers.

Thus, for the first of these, in epistemology exactly as in ethics, freedom is a very uncertain term. It may mean, and there is always temptation to make it mean, absolute independence and isolation, in short the freedom of indifference; or it may mean, and I think it should mean, only clearly recognized and so clearly defined relationship, the freedom of well-controlled interest and responsibility. The former meaning, however, is that into which Professor Münsterberg's zeal for a freed psychology is constantly leading him. Even in his important chapter, 'Die Psychologie und das praktische Leben,' where the position is by no means so extreme as in the book, *Psychology and Life*, the impression given is that of freedom through divorce rather

than through defined relation, for life as 'real life' does not seem to have any vital need of science in general or of psychology in particular. Successful knowers of men are represented as in no sense psychologists and as not needing to be psychologists. Lawyers and teachers and preachers, too, are similarly independent. And yet we read: "In jeder Situation die [zahllosen sozialen] Verhältnisse es uns aufzwingen, den anderen schlechthin nur als ein Objekt * * * zu betrachten, und nur die Psychologie darf dann unsere Schritte lenken. So lange wir aber den Freund oder Gegner, den Kunden oder den Mitbürger, den Belehrung Suchenden oder den Trost Suchenden als wollende Persönlichkeit anerkennen, liegt seine geistige Wirklichkeit in einer Sphäre, für welche die Kategorien der zerlegenden und erklärenden Psychologie grundsätzlich keinen Sinn besitzen" (p. 199). This is most explicit, but it admits, or virtually admits, too much. If every situation in real life does really involve, and for my part I believe that it does, the regard of one's fellow *both as an object*—whence psychology—and *as a willing personality*—whence all the relations of active life, I have to wonder if it is even possible, physically possible or psychologically possible, for teachers and lawyers and others in real life not to be also psychologists or at least in some degree psychological, and on the other hand for psychologists and even epistemologists not to be also men of affairs—perhaps in some cases strenuous teachers and reformers. Certainly things that coëxist, that are coëxtensive, both being present 'in jeder Situation,' must be very intimate with each other; nay, they must be essential to each other, mutually implicative; and even a superhuman agency would put them asunder with greatest difficulty. Moreover, is not the really 'real life,' of which Professor Münsterberg says so much, not something apart from science, but in itself the very conflict, in which he is taking such a conspicuous rôle, between the two views which he has properly enough opposed to each other? In any case, with such an idea of the real life I referred above to the moral purpose and personal spirit of Professor Münsterberg's book as a comment upon (meaning of course a contradiction of) his contentions, and also I objected to any treatment, open or implicit, of science as only an external act of the will, that is to say, as not itself in and of the will of real life.

For my own part I can think of science and 'life' as separated only as the functions of life in general are separated in what the economists know as division of labor. Things that are separated in this way are never without mutual interest and unceasing competition, each having

and asserting a vital relation to the other and neither ever being free from a disposition to trespass upon the other. This competition, moreover, trespassing and all, is of advantage to both. Professor Münsterberg sees only the danger of the sciences, among them psychology, becoming isms, but no danger is ever single. The threatened development of the sciences into mere ologies needs also to be recognized, for an ology is quite as much to be lamented as an ism. Briefly, science is not for its own sake, and fortunately just such vigorous competition as this of Professor Münsterberg's book, in which science is set so completely aloof, is bound—by inducing an equivalent reaction!—to conserve the relation of science and life.

But science in general and psychology in particular are not only said to be objective; they are also necessarily phenomenalist, dealing with logical transformations or unreal constructions; and here there lurks a second danger of Professor Münsterberg's standpoint. Thus, to find in the phenomenism of science an additional reason for the complete abstraction of the real life or the will, or 'das aktuelle Subjekt,' is to forget, in the first place, that science can never be wholly blind to its own phenomenality, and, in the second place, that it is the very nature of consciousness in general, not merely of the keenly logical consciousness of science, to be phenomenalist. Consciousness in general deals, and always must deal, with symbols, constructions, transformations, and also it *knows* that what it deals with is symbolic or constructed. Science is not a peculiar consciousness; it is only the extreme development of something in all consciousness; and as in the general case, so in the special case, the case of science, due allowance has always to be made by the willing subject for the symbolic or phenomenalist character of its objects. Conscious creatures, from the moment they begin to draw breath, are trained to see one thing and understand another, to use what they see as not reality but only a symbol of reality, and so in volition to allow for the unreality of their objects of consciousness; and just this training, I should say, is what has made the high development of science possible and is also what enables and properly controls the application of science to practical life. The phenomenism of science, then, is no reason for its divorce from life; indeed, it is, on the contrary, one of the motives of its application to life, since it must ever awaken within science a spirit of unrest, a longing for reality. Only application can save science from the charge and the conviction of bankruptcy.

The abstraction of science, accordingly, cannot rest merely on its phenomenism nor for that matter on anything else that would seem

to make the scientific consciousness qualitatively peculiar. Is science 'zerlegend und erklärend'? So is consciousness as such; so is all consciousness. Science sees through a microscope, truly, but it still sees; it is moved by the impulses of real life only at long range, but it *is* moved by them; it deals with practical questions, questions of life and death, of personal need and social utility under all sorts of disguises and indirections, in short scholastically, but it does deal with them; and it engages in all of life's conflicts aside and dramatically, but it engages in the conflicts. Am I only fanciful in a way that has no place here when I think that the scientist's laboratory is but a stage on which in various forms and under various conditions, minutely reproduced and carefully organized, man watches the drama of his own life? Well and good. Let it even pass for mere fancy, but at least it hints at a reason for *real* man's absorbing interest in science, and it explains without undoing the abstractness of science and insures as well as justifies the application of science, giving to science as it does the character of only a very critical rehearsal of real life.

The scientific consciousness, too, in a very real sense that has been frequently recognized, is collective or social, and in this character of it one can see a reason both for its objectivity and its phenomenality to the individual, as well as for its exclusion or rather its suppression of the individual will. Kant's *a priori*, for example, so closely related to all modern scientific phenomenalism, has again and again been taken as only his way of giving account of the social mind or say of the abstracted social factor of individual experience. With this understanding, however, the development of an objective and phenomenistic science at any time should be accompanied by social integration and should be a forerunner of some new social movement, the individual will losing itself in the more inclusive process that science has been describing and explaining. Be this as it may, however, suffice it now to say that no theory of knowledge can afford to neglect, as on the whole Professor Münsterberg's theory certainly does neglect, the opportunity for explaining science which the social factor in experience undoubtedly offers.

And furthermore I cannot but think that a keener appreciation of this social factor would have saved Professor Münsterberg from treating not merely psychology and physics but also psychology and both the historical sciences and the normative sciences as at once separate and coördinate sciences. Objectively, perhaps, the sciences are coördinate, just as objectively one might even say that all the parts of the body are of equal rank, being all essential to its integrity; but equality,

like freedom, is a troublesome conception, being too easily taken, among other things, to mean sheer individualism. In reality must not coördination or equality comport with the subjection of each individual to all the others? Science to-day—and this I think is constantly getting truer—is no mere aggregation of separate inquiries, each with its own ‘free’ or distinct object; it is coöperative. Professor Münsterberg would believe that physics and psychology are different in the way already outlined here, psychology dealing with individual objects and physics with universal or ‘over-individual’ objects, and that the historical and normative sciences deal not with objects at all but with will-acts, with individual and ‘over-individual’ will-acts respectively; but such distinctions, full of suggestion as they are, epistemologically worth while as they are, instead of separating the sciences only make them all the more indispensable to each other. Reality, which is divisible in no such way, in spite of, or rather because of, the phenomenalism, is a passion even of science, and is bound therefore to make any special science whatsoever consciously dependent on every other and so prone to use both the methods and the material of every other in its own researches. So psychology uses and needs to use the physical sciences, which strangely enough are most opposed to it, and this use and the need of it Professor Münsterberg recognizes and insists upon emphatically; a psycho-physical parallelism is *epistemologically* necessary to a science of psychology; but does Professor Münsterberg properly appreciate what he recognizes? Is the need only epistemological? For my own part I think not.

Furthermore, apart from the relation of the physical sciences to psychology, history at the present time is notoriously not a science by itself but only a method in science. Even history in the narrow sense of a study of human progress and achievement is after all only a method of general political science, while the ‘historical method’ is a resort of every scientific investigation. In general, therefore, because the sciences are differentiated only to become methods of each other, they are not to be treated in the individualistic way—certainly individualism is his strong leaning—of Professor Münsterberg, but rather they are to be regarded as one and inseparable—and are made so by their very differentiation. How else can we give meaning to such characteristic developments of modern life as physiological psychology, physiological chemistry, physical chemistry, evolution-mechanics (*Entwickelungsmechanik*), and the like? The time has come when scientific specialism by some grim fate or other has actually turned all the different sciences into methods of each other,

and this development, properly appreciated, certainly makes both their separation and their relation something more than an epistemological affair, since it evidently brings the will of real life into the very camp of objective science. Something like a feeling for just this I have liked to read between Professor Münsterberg's lines, but he himself, I fear, would find such a meaning only perversion or misinterpretation, since it makes the volitional attitude and the scientific attitude altogether too intimate. It makes them intimate, of course, because just the will which is necessary to the application of a phenomenalist science to life, and which from its first breath has been trained to make allowance for phenomenism, is a necessity, is even all the more a necessity, to the application of one phenomenal science to another. Morally, or *even* metaphysically, as well as epistemologically, the separation of the sciences is of significance. An epistemologist has no right to expect to escape metaphysical criticism.

But what is to be said in particular of the separation of the objective sciences and the normative sciences, say for example of psychology and ethics? We have seen in the foregoing that the very knowledge which science has of its own phenomenism only shows the will of real life lurking even in the scientific consciousness, as if the scientist, however apparently 'objective' and 'special,' were really in sympathy with something like the mediæval doctrine of reservation, and we have seen too that in the relation of the different sciences, using each other as they do and must, the will of real life is very much in evidence, and now in this matter of the relation of psychology and ethics something similar is to be discovered. Ethics may be one thing and psychology may be another; the two may be very different things indeed, as different even as real life and objective science; but put them side by side, make them contemporary and aware of each other, as any one who would separate them however sharply, even relegating them to different classes or castes of society, certainly must, and their propinquity is sure to beget a mutual interest—I almost said love. Ethics as *only* a normative science, not an objective science, can not fail to entertain doubts of herself and out of her helplessness and insufficiency at last to look with quiet appeal at psychology, and the psychology that pays no heed is—well, it is, as Professor Münsterberg would seem to wish it, lacking in common humanity. But, apart from the romance in the relation of these two, my simple point is this. Psychology and ethics, knowing their limitations, can not avoid, and they have a right to express a conscious

dependence on each other. An only normative ethics must and does feel that its rules are provisional or hypothetical, its standards arbitrary and its outlook narrow, and an only objective psychology must and does chafe under its peculiar world as a mere idea, and the relation between them, that the dissatisfaction of each begets, is a *real reality* of the 'real life.'

Professor Münsterberg's separation of the objective and normative sciences is dangerously near to being only the old story of a science as teaching us to know and an art as teaching us to do. This story could pass muster in the days of 'double truth' and 'double life,' but in these times, when dualism with all its incidents is at best only an epistemological fiction and open to question even as that, the story must take its place among interesting myths. Art is action; science is knowledge; and all knowledge is knowledge how as well as knowledge what. Professor Münsterberg would perhaps admit all this, and his book in spirit if not always in letter has seemed to me to recognize the real value of science to life. Certainly he does not mean to say that science is a luxury, an epiphenomenon, and yet too often he seems to say this and will, I am led to believe, be so interpreted by many who read him. Thus, to review what has been suggested here, he writes of the freedom of the object in a way that is, to say the least, ambiguous; he does not appear fully to appreciate the import of science being not merely phenomenalistic but also awake to its phenomenalism, that is, of its being qualitatively one with consciousness in general and so quite as much a volitional as an objectively scientific attitude; he fails really to use the opportunity for explaining science's objectivity and phenomenality which is afforded by the social factor, or better the social will, in individual experience; he only partially apprehends the significance of specialism, in which the different sciences are seen to have become methods of each other and in which accordingly the will of real life is manifestly active; and he is blind to the romance always going on between the normative and objective sciences. This is truly a rather long list of criticisms, but lists are easily made and of course the foregoing comprises really only one criticism more or less extensively analyzed. Perhaps the one criticism is this: Professor Münsterberg's epistemology is not sufficiently responsible to metaphysics. *A theory of knowledge and a doctrine of substance are mutually dependent.*

But, finally, there is the *Aktionstheorie*, which is full of interest and is perhaps the most satisfying thing in a most interesting book. As was noted above, this theory is comprised in the concluding chap-

ters of the third part, 'Der psychische Zusammenhang.' To speak after the analogy of a circumference, its three determining points are as follows: (1) "Jedes motorische Centrum steht zunächst zu einem antagonistischen Centrum in Beziehung" (p. 534). Every action, this amounts to saying, has its specific reaction,¹ and very much as the equation of action and reaction in the physical world gives control or conservation, as something immanent instead of imposed from without or instead of resident in any single mass, so here in the sphere of the psychical control is made immanent. Thus: (2) "Das motorische System ist also kein Armeekorps mit einem einzigen General an der Spitze; es hat viel mehr Generale als Gemeine, aber die Anordnung in übergeordnete und untergeordnete Funktionssysteme muss unbedingt festgehalten werden" (p. 533). So would psychology or physiology apply within its sphere of interest and influence the principles of democracy in place of those of militarism and monarchy. But (3) we read: "Die Aktionstheorie * * * wird * * * behaupten, dass es überhaupt keine Empfindung giebt, der nicht ein motorischer Impuls zu Grunde läge und dass somit in diesem Sinne jede einzige Empfindung und daher jedes Element des gesammten Bewusstseinsinhaltes eine Innervationsempfindung sei" (p. 529), and this third point, explicit as it is, certainly completes the definition of the theory. Moreover, besides completing the theory it is dangerously near to betraying its author's earlier epistemology. Thus who can help jumping in his thought from consciousness as 'Empfindung' to consciousness as science or from 'Innervationsempfindung' to the will of real life? The chasm between them has never been impassable. And who can help thinking not only that the distinction between 'Empfindung' and 'motorischer Impuls' is parallel to that between science and 'real life,' but also that the difference between the sensory system and the motor system is in some way intimately involved in that between 'jedes motorische Centrum' and its inevitable antagonist? 'Real life,' in a word, although merely as physiologists or psychologists we may not call it that, must be supposed to have its special organs, its special localization, in the body and to admit it there even under a scientific *incognito*—so I have to think—is seriously to complicate the consistency of Professor Münsterberg's position. Again, to call the *Aktionstheorie* only a logical construction in science, a mere theory of physiology or psychology, is as far from saving it from being made a theory of real life as the cloister was from protecting nominalism against the real life of religion and politics.

¹ That is, counteraction.

And the *Aktionstheorie*, with its suggestion of control through the opposition of centers—analogue, as hinted already, to physical conservation through the equivalence of action and reaction—is not without a very decided suspicion of Hegelianism, and we have to wonder if Professor Münsterberg is not in danger, in spite of himself, of being carried over to an avowal of relationism and incidentally to the theory of interaction. Certainly, the *Aktionstheorie* is not dualistic in its spirit, whatever it may be in form. This reading of it, too, makes one able to gratify Professor Münsterberg by associating him with Fichte, although throughout the earlier parts of the book the presence of Kant or even of Schopenhauer is felt, and felt strongly. In the preface (vii, viii) we are told that the present book hopes once for all to banish the ghost of Schopenhauer, which so many saw with their very eyes in *Psychology and Life*, and that, historically, the author would prefer the company of Fichte; but this can only mean either that I have not understood the book itself and so that the form of statement is defective, or else that in his concluding chapter the author has really outdone himself.

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Naturalism and Agnosticism. The Gifford Lectures delivered before the University of Aberdeen in the years 1896-98. By JAMES WARD, Sc.D., Hon. LL.D., Edinburgh, Professor of Mental Philosophy in the University of Cambridge. Two volumes. New York, The Macmillan Co. 1899.

We are due Professor Ward an apology for the tardy appearance of this notice of his important work. Suffice it to say that circumstances over which the editors had no control, and lying without the knowledge of the present writer, rendered the delay unavoidable.

At this late date it is probably unnecessary to set forth the contents of the work in great detail. One may recall that the lectures deal *critically* with (1) the Mechanical Theory; (2) the Theory of Mechanical Evolution; (3) Biological Evolution as involved, though not always expressed, in the conclusions of Lamarck, Charles Darwin and their followers; (4) the Theory of Psychophysical Parallelism, with its accompaniments, 'mind-stuff,' 'double aspect,' 'conscious automaton' and 'mind as an epiphenomenon of matter.' These critical and destructive excursus are followed by a 'Refutation of Dualism,' in which Professor Ward indicates his own standpoint and his reasons for adopting it; this he accomplishes here by the double refraction process mainly. Finally, a theory, called 'Spiritualistic Monism,' is

advanced; this furnishes forth the lecturer's positive construction, in so far as he finds himself in a position to formulate basal principles.

Despite Dr. Ward's apologetic preface, which applies, no doubt, far more to the constructive than to the critical sections of his work, these lectures must be viewed as a contribution of capital importance for contemporary thought, especially in the English-speaking lands. Moreover, they are of particular interest to the American public, for academic thought in the United States happens to be in a more fluid condition at the moment than in Britain. This peculiar interest must be traced to the fact that our author possesses competent knowledge of science, and perceives, as too few British writers have perceived, the vast import of scientific advances and conclusions for philosophical inquiry. As contrasted with Germany, Britain and the United States remained outside the main current of nineteenth century progress in speculation for nearly three generations. If we take Herder's 'Ideen' (1784-89) as the first conscious embodiment of what was to become the nineteenth century standpoint—historico-evolutionary, we find that, as concerns systematic treatment, the English world remained barren till 1865, when Dr. Hutchison Stirling burst upon it with his 'Secret of Hegel.' During this long interval, the British contented themselves with the quasi-political principles of utilitarianism which, although sensing the organic conception of society—not of the universe—in some ways, remained for the most part under the domination of the static ideas of the seventeenth and eighteenth centuries; with the localized problems of the Scottish school, in which speculation was determined largely by provincial conditions; with the ineptitudes of Mansel, whose fulminations can hardly be said to display vital comprehension of the achievements of Kant and his successors. No doubt Coleridge and Carlyle, to name no others, furnished an earnest of better things. But were they not mystics and misbelievers, perhaps disreputable personages? Accordingly, when Hegel did make entry, his pregnant ideas served to fill an aching void, were thus seized with intense avidity, and transformed into a veritable gospel whose converts still bear strong, sometimes haughty, sway.

Similarly in the United States. Emerson and the transcendentalists, thanks to New England Unitarianism, received more immediate response than Carlyle. But, as strict philosophy went, thanks this time to the denominational colleges, the eighteenth century modes of the Scottish school were impressed upon the callow youth till the seventies. While, after 1875, the Hegelianism of St. Louis and of several among the rapidly growing universities played a part not un-

like that taken in Britain by the idealistic groups at Glasgow and Oxford. Here the parallelism ceased. For, dismissing the consideration that it is easier to dominate a small than a large country, Britain offers no counterpart to the wonderful development of experimental psychology which has effected so much in the United States for a better understanding between science and philosophy. Further, as the American universities maintain intimate relations with those of Germany to a degree unexampled in Britain, the later philosophical movement at Halle, Bonn, Leipzig and the rest—positive and philosophical in character—has had larger influence over cisatlantic conditions. For these reasons Dr. Ward's work may well receive greater approval and attention here than in the land of its birth. Further, it betrays something too much of Lotze's influence to be altogether palatable to the dominant British school. While in this country Lotze's system, evil though its substantialism has been in many respects, counts students enough to be sure of a respectful hearing. On the whole, then, the office of these Gifford Lectures, as the present writer conceives it, is to bring us to a consciousness of the present relation between science and philosophy, a relation grasped more vividly, maybe, by the American than by the British reader.

In his discussion and evaluation of exact science, strictly so called, Dr. Ward shows with great success that modern dynamics and mechanics involve fundamental philosophic (metaphysical and epistemological) presuppositions. Only when these have been separated out from the proper details of the sciences does it become possible to set forth the precise implications adopted *ab initio*. In other words, in so far as the exact sciences have come to contemplate latterly a theory of the universe, rather than a mathematical description of certain aspects of human experience, they have laid themselves open to criticism, to basal valuation, at the hands of philosophy. Thus it becomes simple to trace the reasons for their inadequacy, not within their own chosen, or abstracted, spheres, with which philosophy has nothing to do, but in their explanation of the universe based upon foundations limited artificially to meet the requirements of the content of these sciences. At best, as Dr. Ward proves, we get nothing more than description here; at worst, a series of analogies wherein, from certain 'fictions of the understanding,' a passage is made to ultimate reality. In short, unvalued postulates, not rationally defended principles, govern the cosmic construction.

So much attention has been attracted by the criticism of Mr. Herbert Spencer, in the lectures on the Mechanical Theory of Evo-

lution, that one need do no more than advert to it. I find myself in complete agreement with the majority of Dr. Ward's strictures, especially in that they bring us to clear consciousness of the fact that Mr. Spencer's work has relapsed into a position of historical value only for some years back. On the other hand, the criticism of Biological Evolution is by no means so successful. One reason for this is its extreme brevity. Thirty pages do not afford a canvas large enough for treatment of the 'Lamarckian, Darwinian and ultra-Darwinian theories,' even 'generally,' as the caption runs. In particular, I take it that cautious scepticism is well in place regarding Dr. Ward's elevation of 'subjective selection' to a position of such importance. And, apart altogether from difficulties in the matter itself, these sections are unfortunate as tending to produce misunderstanding on the part of professional biologists, one of whom, for example, reminds our author 'that nothing is easier than for one who is not a naturalist to improve upon the work of Charles Darwin.' Remembering the office which, as indicated above, this work is so well fitted to subserve, one cannot but regret that the enemy—who is really a friend—should find any cause to blaspheme. The truth is that a more extended and thorough treatment of biological evolution from Dr. Ward's pen were very greatly to be desired. In especial, the 'teleological factor'—Lamarckian—in the origin of species demands fresh and further interpretation. For, as it stands now, it seems like to cause a serious, and probably unnecessary, difference between scientific men and philosophers in their consideration of what, after all, cannot but be an identical problem for both.

The first part of the second volume is devoted to the discussion of the Theory of Psychophysical Parallelism. In these pages our author devotes attention to Spinoza, Clifford, Huxley, Du Bois-Reymond, rather than to such psychological writers as Bain, Höffding, Lange, Münsterberg, Jodl, and their fellows. There is a reference to Wundt's article on Parallelism (*Phil. Studien*, x, 33), but to the simple effect that its conclusions are methodological, not systematico-constructive. As against the thinkers with whom he thus takes issue, Dr. Ward is entirely successful. It is interesting to note that he indicates his agreement with James by some pertinent quotations. But here again, as in the case of biological evolution, we would like something bearing on those more recent phases of the controversy, in which criticism of the parallelistic theory has been undertaken by such writers as Busse, Rickert, Wentscher, Erhardt and others. At bottom, the problem is, of course, a metaphysical one. But it cannot be

solved, or even palliated, till we have worked through, not merely the epistemological 'inhibitions,' as one might term them, but also through the physiological and psychological facts in detail. It were much to be desired that a master like Dr. Ward, equipped, not merely with the requisite knowledge, but with a peculiar detachment, insight and cool acuteness, should return to this problem in its most recent phases. I, for one, must hold that, till some thorough treatment be forthcoming, monistic construction cannot avoid the pitfalls of otiose acquiescence. And it is by no means certain that the 'Spiritualistic Monism,' offered by Dr. Ward as his final solution, is free entirely from this very limitation.

Yet, whatever may be said in the way of criticism, one would fall short of his duty did he fail to remind readers that, taken as a whole, these Gifford Lectures must be classed with the most important British contributions to philosophy in recent years—with Green's 'Prolegomena to Ethics,' E. Caird's 'Kant,' F. H. Bradley's 'Appearance and Reality.' True, the office of these last is different, and their initial audience was not of the same character. Nevertheless, in what Dr. Ward implies, more perhaps than in what he actually states, he deserves the closest attention of philosophical students, more especially of those who hold, like the present writer, that the immediate future of philosophy depends on the manner in which it reckons with the positive sciences.

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COMPARATIVE PSYCHOLOGY.

Animal Behaviour. By C. LLOYD MORGAN, F.R.S. Illustrated. London, Edward Arnold. 1900.

The author set out with the purpose of preparing a new edition of his 'Animal Life and Intelligence,' but concluded, and all will agree wisely, to write a new book, into which very little material from the above or his other works has been introduced.

The chapters on organic behavior of plants and animals, with pictorial illustrations, are written in order to show among other things that while there is beautiful adaptation effective to a definite biological end, there is an absence of a guiding consciousness. Among plants there is no evidence of profiting by experience. After a glance at reflex action the discussion of the evolution of organic behavior is considered. To the question, Are acquired modes of behavior inherited? a negative answer is provisionally given, though it is admitted

that if this be not the case 'the method of natural selection in racial progress is curiously indirect.'

Natural selection develops congenital definiteness of response and such innate plasticity as is of advantage under the conditions of existence, uniform conditions tending to emphasize the former, variable conditions the latter.

Professor Morgan concludes that a belief in an accompanying consciousness in the organic behavior of animals is wise, since the associations which take part in the guidance of behavior are so varied and delicate that a skeptical attitude is a greater strain than is a belief in conscious control. And if his remark were extended it would explain very well the attitude of some who are disposed to be somewhat more conservative in regard to many points in animal behavior than Professor Morgan and those of his way of thinking.

Speaking of the explanation of the behavior of a chick, after some experience, the author well says: "Sentience is not sufficient for guidance; there must be consentience involving the presence of several elements; they form constituent parts of the coalescent situation as a whole, of which alone the chick is personally conscious, without analysis of detail," though it is felt that the manner in which consciousness affects behavior is far from clear.

Briefly, consciousness in the first stage of development may be regarded as an accompaniment, in the second as a guide, and in the third as a judge. The latter must of course not be attributed to animals—it is characteristic of man alone.

Instinctive behavior is treated at considerable length. Mr. Marshall's views are subjected to a critical examination, with the general result that while Professor Morgan agrees that instinctive acts tend to the well-being of the individual and the preservation of the species he does not hold that the biological end is the objective mark of an instinct. On the other hand approval is expressed of Dr. Peckham's definition of instinctive behavior: "All complex acts which are performed previous to experience, and *in a similar manner by all members of the same sex and race.*"

Professor Morgan himself would define instinctive behavior as "comprising those complex groups of coördinated acts which are, on their first occurrence, independent of experience; which tend to the well-being of the individual and the preservation of the race; which are due to the coöperation of external and internal stimuli; which are similarly performed by all the members of the same more or less restricted group of animals; but which are subject to variation, and

to subsequent modification under the guidance of experience." Most readers will agree that this leaves little if anything to be desired as a general statement of the case.

Then follows an examination of the subject as it is illustrated by insects and birds.

The characteristics of instinctive behavior in birds are the following:

1. That which is inherited is essentially a motor response or train of such responses. The compound reflex action of Herbert Spencer.

2. These often show very accurate and nicely-adjusted hereditary coördinations.

3. They are evoked by stimuli, the general type of which is fairly definite, and may in some cases be in response to particular objects.

4. They are also generally shown under conditions which lead us to infer the presence of an internal factor, emotional or other.

5. There does not seem to be any evidence of inherited knowledge or experience.

While there is probably in all cases present some internal prompting, it is not equally clear whether a definite external stimulus is invariably necessary. If the latter is indispensable, the reviewer can testify that in some instances 'definite' must be given a very liberal meaning, if not replaced by another term. A very small spark indeed in some cases—if spark at all—is required for the combustion which sometimes seems to be all but spontaneous.

The author does not look with favor on the term 'acquired instincts' of Wundt.

"How comes it, then, that the chick does not instinctively respond by appropriate behavior to the sight of water?" asks the author. Some of us would question this. We hold that the chick does respond to the sight of water under the conditions of its normal existence and frequently under the more or less unnatural conditions we substitute. It is merely a case of a less certain or more tardy response than that to food, so that arguments founded on this supposed fact must carry little weight, whether applied to heredity or other problems.

While frankly admitting that a perfectly satisfactory explanation of intelligent behavior cannot be given at present, Professor Morgan thinks that "from all parts of the automatically working organic machine messages come in to the center of conscious control, and that in accordance with the net result of all these messages, past and present, tinged with pleasure or pain, other messages go out to the

automatic centers, and by checking their action here and enforcing it there, give new direction to resulting behavior."

The physiologists and neurologists at all events will approve of this view of the case, and probably the psychologists equally. When, however, the author goes so far as to say, in comparing intelligent and instinctive behavior, that "in performing the instinctive act, the animal seems to have no more individuality or originality than a piece of adequately wound clockwork," we must dissent. This is far too strongly put for the instincts of the higher animals at least.

Professor Morgan makes generous use of the valuable researches of the Peckhams. Speaking of certain behavior of the wasp, he says: "Here we have intelligent behavior rising to a level to which some would apply the term rational. For the act may be held to afford evidence of the perception of the relation of the means employed to an end to be attained, and some general conception of purpose."

The author thinks that the question as to whether animal intelligence attains to the 'rational' is more likely to be answered through experiment than by chance observations.

We would say both by experiments and by *carefully made observations*, of which the latter would likely prove the more important, as being more likely to find the animal under natural conditions.

Professor Morgan seems to have based his conclusions in regard to the intelligence of animals, especially dogs, on some observations or experiments with one or two dogs he possessed. He lays great stress on several failures of his fox terrier to get a stick through a fence. Would a well-trained retriever, collie or poodle have had a like difficulty—although their experience had not been in the direction referred to in this case? The reviewer's study of many breeds of dogs leads him to take a much higher view of their intelligence than Professor Morgan seems to believe justifiable. His experience with throwing a ball has been very instructive. In this he has generally tried experiments, sometimes at the same time, with a half dozen dogs of dissimilar breeds. They behaved very differently—perhaps more so than the same number of small boys would have done. The St. Bernard showed a great deal the most of what one might call 'gumption.' He took in the situation by far the best, and adapted to it in a surprising way, while the terriers were altogether more machine-like.

Dr. Thorndike's experiments are reviewed and criticised somewhat unfavorably, though Professor Morgan in many of his positions on the subject of animal intelligence does not differ greatly from Dr.

Thorndike. Says the author, commenting on the behavior of his dog in lifting the latch of the gate: "He did it with the back of his head. I could not get him to do it (more gracefully) with his muzzle." And why should he, seeing that the muzzle is a very sensitive part in dogs?

The author holds that "it may be said that between the method of intelligence and that of fully developed rational procedure there is a wide gap which must have been bridged over in the course of mental evolution. Unquestionably, and in contending that the methods of the animal are predominantly intelligent, I am far from wishing to assert dogmatically that in no animals are there even the beginnings of a rational scheme. * * * We shall probably have to await the further results which must be the outcome of patient and well-directed child-study."

Naturally the reviewer read the above with hopeful satisfaction. With Professor Morgan the case against rational behavior is, at least, not yet closed.

It is believed that growth in intelligence takes place by what the author designates as 'condensation of experience by an elimination of detail and the survival of essential features'—also by the elimination of those modes of behavior which were not efficacious, *i. e.*, by the functional selection of Baldwin. According to this view an animal may come very near to the attainment of the abstract without quite reaching it.

With the development of the higher intelligence instincts decay, which possibly explains why man has so few 'stereotyped instincts'; nevertheless residua remain, which explains much.

Under 'Imitation' Baldwin's 'circular process' is discussed and its truth admitted with certain important reservations; but Professor Morgan does not favor on the whole the extended usage of the term that Baldwin would advocate.

On the other hand the author cannot endorse Dr. Thorndike in his extreme position on the subject of imitation, although he does not apparently believe in 'reflective imitation,' though 'intelligent imitation' is conceived to be of great importance, as also 'instinctive imitation.' In ants, difference in behavior is thought to give rise to suggestive effects on the other members of the community, rather than that their conduct is dependent on communication by any definite system of signs; nor does the author believe that dogs understand words in the proper sense of the term.

While in general Professor Morgan is not convinced that animals reason, he is not prepared to assert dogmatically that they do not, for

he says: "Presumably in the ant, rook, and beaver anything like an ideal scheme of thought based on reflection, if it exist, is as yet exceedingly indefinite."

Under 'Play and Courtship' the views of Professor Groos are considered, and the author would have the former attach more importance to courtship in generating and strengthening the ardor of the male. Nor would Professor Morgan think it necessary to introduce 'anything so psychologically complex as the conscious illusion of make-believe' in order to explain certain forms of mock combat, etc. He would differ from those who hold that play is always the outcome of a surplus of energy. He instances the case of a sick kitten attempting to play as evidence to the contrary.

The infrequency with which the term 'association of ideas,' apparently used by many to cover psychological ignorance, occurs in this work is noteworthy.

One lays down this volume with the feeling that it is a psychological and literary production of an unusually high order.

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'The Fur Seals and Fur Seal Islands of the North Pacific Ocean.'

Part III. Pp. 69-74. Washington. 1899.

These papers, which are either mere notes of isolated observations or minor experimental studies, are of interest as indicating a growing disposition on the part of naturalists to study living animals from a more or less psychological point of view, rather than for any definite results of importance to psychology. Two of them (by trained psychologists) are especially noteworthy because of their employment of a simple experimental method, shown to yield highly instructive results.

Dr. Thorndike used the common *Fundulus*—a bony fish, lacking a brain cortex—in order to test 'the ability of fishes to profit by experience and fit their behavior to situations unprovided for by their innate nervous equipment.' The device used was a large aquarium. The space at one end was shaded, and all food was administered at this end. Along each side were fastened simple pairs of cleats fitted to receive partitions (of wood, glass or wire screening) made each with an opening sufficiently large for the fish to swim through. After a fish had been driven to the illuminated end, a partition was introduced, and its reactions to the situation observed. In its effort to reach the dark, the fish swam repeatedly against the screen, until it chanced to find the opening, through which it swam unimpeded to the shaded end, in which it was allowed to rest for some time. As the experiment was repeated, the fish tended to make fewer mistakes, fewer pauses, until finally the appropriate movement was directly made, and without delay. Several individuals were tried with this and similar experiments (*e. g.*, one in which three slides were used, thus involving a serial multiplication of associations) with a like result. Figures and dimensions of the apparatus are given.

Dr. McIntosh, Professor of Natural History in the University of St. Andrews, communicates a number of interesting miscellaneous observations on several English fishes, which indicate their ability to profit by experience, and that rather rapidly. Such facts are familiar to every fisherman.

Dr. Thorndike's experiments are valuable as providing at once a demonstration and a measure of the associative process.

Mr. Yerkes made experiments of a similar nature upon a single individual of *Chelopus guttatus*, the common 'speckled turtle.' The turtle 'learned' to find its way through a labyrinth, arranged by dividing a box by means of partitions provided with openings so placed

as to compel the animal to take a somewhat irregular route in order to reach the dark 'nest' of damp grass. When placed for the first time at the farther end, the turtle reacted to the unfavorable situation of confinement, light, etc., by 'wandering about almost constantly for thirty-five minutes,' at the end of which time 'it chanced to find the nest, into which it immediately crawled, there remaining until taken out for another experiment two hours later.' After the first three times, the routes, which had been extremely tortuous, became much more direct. Fifty trials were made. The times and tracings of the paths show a rapidly increasing economy of movement. "What at first took minutes, after a few trials took only as many seconds." The sense of smell may have served as a guide. Analogous results were obtained with a complication of the labyrinth by the addition of a blind alley and three inclined planes. In the second labyrinth the turtle gradually learned to shorten its path eight or ten inches by taking a fall (of four inches) over the edge of an incline. Everything in these experiments, it is wisely argued, points conclusively to mere associative capacity to profit by *chance* experience. This popular article is clearly written and illustrated with diagrams. While the results are extremely suggestive, they require to be supplemented by extended experimentation.

The Peckhams, who have rendered a signal service to comparative psychology by their publications on spiders, wasps, etc.—the fruits of enthusiastic zeal and patient industry in observation, together with ingenious reasoning—in the note cited record the 'change in a long-established custom' of the wasp *Sphex ichneumonea*, which they attribute to 'reason.' The animal, at least, is thought to have learned finally to act at variance with the deep-rooted instinct of leaving her prey (grasshopper) at the entrance to her tunnel and then running in and out again before dragging it down, after repeated experiences of finding it at a changed position (due to the disturbing hand of the experimenter). There is not space to discuss it here, but the evidence as stated in this single case, does not seem to us conclusive.

Mr. Shufeldt, after commenting on Professor Whitman's "Animal Behavior,"¹ discusses some of the sense powers of teleost fishes and describes certain observations on living fishes in an aquarium. The peculiar behavior of the young of the snowy grouper (*Epinephelus niveatus*) and the big eye (*Pseudopriacanthus altus*) when ap-

¹Biological Lectures from the Marine Biological Laboratory, Woods Holl, Mass. (Boston, 1899.)

proached, or under certain other conditions, is explained as an instinctive simulation of death or dying and accorded selective value.

Messrs. Young and Cole record some interesting observations in the methods of nest-building in the brook-lamprey which seem to indicate a high degree of variability in the instincts involved, and which are, therefore, of some interest as data for a genetic theory of instinct.

A like variability on the part of certain birds (notably the robin, cedar waxwing and red-eyed vireo) in respect of their partial habit of swallowing the excrement of their young by way of cleaning the nest, is noted by Mr. Herrick, who suggests that it may serve as food for the parent, since digestion in the young is an imperfect process. Other precautions on the part of the adult bird are mentioned as definite and invariable instincts.

The late Professor Norman's preliminary notes on the habits of *Typhlomolge rathbuni*, posthumously published by Dr. C. H. Eigenmann, contain some observations on the sense powers of this species. The individuals tested showed no reaction against light. The sense of touch seemed highly developed, but could not be experimentally localized. There was no evidence of a sense of smell. The paper is a mere fragment.

Mr. Lucas, 'after a careful study of the behavior of the seal in the field' has been impressed with the absence of intelligence in this animal, which he regards as conspicuous for the blind stability of its instincts. "As it has done in the past so it will do in the future. Its habits, being formed by the slow process of natural selection, can change but slowly, hence the fur seal is not likely to alter its habits, or to adapt itself to changes in surrounding conditions. It may be exterminated, but it will not leave its breeding grounds, and the last seal will come calmly on shore to be knocked in the head." Which would seem of more moment to economics than to psychology.

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FEELING.

Die Gemüthsbewegungen und ihre Beherrschung. C. M. GIESLER. Leipzig, Barth. 1900. Pp. 68.

The aim of this discussion is practical, to control the emotional life. The more we avoid intense and exciting emotions, the longer we shall be able to endure the strains of a life that becomes ever more complex and strenuous. Before we can take up the practical problem, however, many theoretical questions must first be discussed, and in

the first of the six brief chapters various views as to the nature of emotion are mentioned. The author's comments upon these views are almost too brief to be called discussions: I. Psychological definitions. Among these, the first group reduce effective states to other forms of mental life, as the definitions of Wolff, Hobbes, Kant, Herbart. The second group represent emotion as an interaction between a mental image (*Vorstellung*) and a feeling based upon a judgment as to the beneficial or injurious relations of the thing imaged to the individual. Here belong the definitions of Wundt and Stumpf. II. Physiological definitions, reducing emotion to sensations caused by the physiological changes that follow the perception of the exciting object. Here are mentioned Féré, James, Lange and Ribot. III. Psycho-physiological definitions, which, like the genetic definitions of Nalowsky and Alfr. Lehmann, conceive emotion as a complex of feeling and organic sensations. Emotion is a union of feeling and organic sensations, the latter being due partly to the automatic excitation of bodily changes, which always take place involuntarily, and partly to undirected and involuntary innervations of voluntary muscles. This view underlies the further discussion of the author.

Chapter two deals with irritability as the forerunner of emotion. Irritability and its accompaniment, contractility, together play the same rôle in the lower animals that emotion plays in the higher. Affective experience is the first form of experience in the history of the race, the prostadium for the growth of intellectual processes. In the child ordinary impressions are in time built into objects, and corresponding to this process in the race is the development of the sense-organs. 1. In the service of self-preservation, irritability is what some authors have, I believe, agreed to call *mimetism*—a reaction which tends to repeat its own stimulus (Baldwin). 2. In the growth of a sense of the external world, irritability and impulse furnish, respectively, the material and the formal elements, and the development of these elements runs parallel with the localization of irritability. Dr. Giessler follows the gradual localization of irritability among the lower animals into some detail.

Chapter three deals with the general theory of consciousness, in order to bring out the importance of movement and of motor images and tendencies in all forms of mentality. Quoting from v. Tschisch, "Sensations of movement constitute an indispensable element in every impression, every representation and every abstract idea."

Chapter four contains an analysis of affective states, in which the author develops the conception of a *diremption-threshold*. Affective

states are at first only feeling, but directly we become conscious of motor excitations which are not regulated by the will. These excitations are followed by a greater concentration upon the object, and for a time a voluntary control is exercised over the motor and ideo-motor tendencies. This is the period of tension (*Spannung*): it is followed by the period of discharge (*Entladung*): and between the two lies the diremption-threshold. There is a vital or under-will, a function of the physiological ego, which is instinctive and serves the ends of self-preservation and self-advancement, and there is also a cortical or over-will, which is guided more by reflection and is a function of the psychological ego. Ordinarily the former is subordinate to the latter, but in all intense emotions the lower instinctive will tends to predominate over the reflective will, and the moment when this tendency is realized, when the compensating influence of the will over the motor and ideo-motor processes ceases to be adequate, is the diremption-threshold. The locus of this threshold depends, (1) upon the relative automaticity and excitability of the motor processes, and (2) upon the relative strength of the apperceptive organ and function.

Chapter five deals with precautionary measures to avoid intense and exciting emotions by limiting the sphere of automaticity. The degree of physiological excitability should be kept down, and one should adopt such a method of life that there will be few if any occasions for the appearance of powerful emotions. Repetition is the chief source of motor excitability, and one should see to it that the automatic motor processes involved in emotion are not repeated very early in life or very often. A consequential habit of acting should be cultivated and altruistic motives should be held in the foreground. So conceive thyself inwardly that thou canst always be true to thyself outwardly.

The sixth and last chapter deals with the positive control of particular emotions. One should cultivate the ability to assume an apathetic attitude toward the object of an emotion. Where an affect involves organic or other sensuous imagery, it may be controlled by occupying the attention with other things. Where the affective state has already reached a high degree of excitement, it can be weakened or suppressed only by another affect, and indeed only by one resting upon ideal feeling. Victory can be won by bringing into the field other categories of worth than those of the moment. Moods may be modified best by awaking æsthetic emotions; affects, by ethical ideals with their emotions; but passions can be transformed or suppressed only by religious emotions.

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Ueber Verallgemeinerung der Gefühle. THEODOR EISENHANS.
Zeitsch. f. Psych., XXII. Pp. 194-217.

The question raised under the above title is briefly this: Do the feelings pass through transformations which resemble the generalization of ideas—are there, that is, ‘general feelings’ in the same way that there are ‘general ideas’? The author finds no difficulty in gathering abundant evidence in support of an affirmative answer to the question. The feelings do, in his opinion, frequently undergo a generalizing process, the results of which are disclosed by certain well-marked characteristics. To enter fully and intimately into the author’s view of how this process is accomplished, one should of course be able to gain first some insight into his general standpoint in reference to the feelings. Unfortunately this standpoint is nowhere clearly revealed. The reader is left to make his own unsatisfactory inferences from the special considerations of the text. And yet, if one is to make an intelligible contribution to our knowledge of the life of feeling, it is certainly most desirable that the reader be first provided with a brief confession of faith. For in the present state of the doctrine of the feelings no contribution to the subject can be correctly understood and properly rated without a knowledge of the general belief from which the view has sprung. The only preliminary supposition that the author sees fit to mention is that there are associations between ideas and feelings, so that when any one of the former is reproduced the corresponding feeling is reproduced also. But this does not admit us very far into the background of the author’s thought. There is, however, one indication of what the latter is. Much of the terminology of the article, many of its illustrations, and some of its divisions irresistibly suggest Wundt and the Wundtian doctrine of the feelings. How far Herr Elsenhans accepts the latter doctrine we have no means of knowing. But this much at least we may conjecture, namely, that he presupposes a *multiplicity of affective qualities*. Whether these qualities are thought of as belonging to more than one affective ‘direction’ is not clear, and indeed is rather indifferent for the present inquiry.

With three guesses at the author’s general position, we may now see how he treats the specific problem before him.

Two possibilities of generalization are conceivable. Either (1) “the feelings participate in the generalizing process of the ideas which they associatively accompany”; or (2) “out of several single feelings there are built up feelings of a more general sort.” Both of these possibilities are realized.

1. The process of passing to a general idea is, of course, that of

reaching a state which may refer indifferently to any one of a multitude of particulars, and which thus comprehends all these particulars within itself. Such an idea is always symbolized by a word, and is represented in consciousness either by a vague and, in a sort, composite image of the individual perceptions or ideas which are included under it, or by a single one of these states, to which the rôle of representative is allowed. How now about the feelings which originally accompanied each of the included ideas? If I rightly understand the author, these feelings are to be regarded as primarily aroused by the representative idea, whose place is in fact so readily taken at any moment by any one of its fellows to which the attention may be turned. Later, however, the verbal expression symbolizing the general idea may itself, by virtue of its close association with the representative image and through it with the feelings, serve to call up the latter directly. Feelings temporally disconnected are thus brought together by the verbal symbol, clustering as it were about this their common center. Thus the expressions 'rustlings of the forest,' 'breezes of spring,' etc., arouse unmistakably the most vivid feeling tones.

But this is only, as it were, the external part of the process of generalization. The essential part relates to two characteristic changes in the feelings. In the first place, the *intensity* of the feelings is reduced to a lower grade; and, in the second place, the *quality* of the feelings, while remaining in general the same, has decreased in definiteness. The intensity of the feelings aroused by the expression 'breezes of spring' is not to be compared with that of those which originally accompanied the single actual experiences. So also the quality of the feelings aroused by the word 'concert' is by no means as definite as is that of those accompanying any single concert heard at a definite time and place. Still, under circumstances, the special idea that stands as representative of its class may carry with it such a clearly marked feeling tone that the quality of the 'general feeling' may be rendered particularly definite and the whole consciousness of the moment be influenced thereby.

The cause of this lessened intensity and diminished definiteness of quality the author seeks, not in the parallel changes that the general idea itself has undergone in becoming general, but rather in the changed teleological relations into which the general idea has come. There is now no such close or definite relation to the weal or woe of the organism, and this fact is immediately reflected in the character of the accompanying feelings. This is the first form in which 'general' feelings may appear.

2. But feelings may come into immediate relations with one another without the aid of a verbal symbol. This may happen when they simultaneously exist. Such single feelings may then be gathered into one of a more general character. A splendid instance of this is the so-called 'common feeling' (*Gemeingefühl*). That this is a *total* feeling, a fusion product made up of numerous partial feelings, is a thought to which Wundt has long since accustomed us. But what the author wishes to emphasize is that the total feeling possesses a *general* character. To be sure, the relation between a 'general feeling' and its constituent parts is not the same as that between a concept and its included ideas. For while any one of the latter may come alone to consciousness, each single feeling has lost its individuality in the fused total. But despite the lack of analogy between the two in this respect the total feeling is seen to possess characteristic marks which make it 'general.' Such are (*a*) the lack of distinct spatial relation to the objective world; (*b*) a qualitative indefiniteness, similar to that noted in the first class of general feelings; and (*c*) no general decrease in the affective intensity, but rather a dependence upon the maximum intensity of any constituent feeling. All these three characteristics are unmistakable in that most comprehensive general feeling that we term 'mood.'

Excellent examples of this kind of generalization are those suggested by Ribot in another connection. (*L'année psychologique*, III., 1-9.) The precipitate of feeling remaining with one after such experiences as travelling through a new country, visiting a cloister, reading a novel, etc., etc., is in every case a 'general feeling' formed out of the more vivid and definite materials furnished from moment to moment by the actual experience. Much the same thing is illustrated by the vague feelings and moods evoked by the modern symbolists.

In the *intellectual* life higher and higher stages of affective generalization may be found, culminating in that feeling which surrounds any successful work of the creative imagination. As to the *æsthetic* feelings, those, for example, that accompany a musical production as a whole are more general than those accompanying specific harmonies and melodies. In the *ethical* life the 'conscience' represents a feeling of general character. And, finally, in the *religious* life such a feeling as that which the idea of devotion arouses is at once seen to be highly composite and general in character.

What is meant by a 'generalizing of the feelings,' and what the two directions are in which such a process may work itself out, may then be clear enough from this brief account. The novelty in this treatment of the feelings is, manifestly, the suggestion that feelings

may come not only into the relation of part to whole, but also into that of particular to general, so that wider and wider fusions of affective complexes may lose more and more the specific and definite characteristics of single feelings. Not all will assent to this way of envisaging the matter, for not every doctrine of the feelings admits of such processes as our author takes for granted. Still, such an attempt as has here been made is certainly a welcome one as assisting, possibly, towards some unanimity of opinion in this difficult and baffling realm of the feelings. It is only to be regretted that Herr Elsenhans has not admitted us more fully and clearly into the several matters of important detail which formed the background for this discussion, in order that we might be sure that the above interpretation of his thought is indeed the correct one.

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La Tristesse et la Joie. By GEORGES DUMAS. Paris, Alcan. 1900. Pp. 426.

Dumas, the translator of Lange's work on the emotions, and the author of another work, 'Les états intellectuels dans la mélancolie,' presents us with a somewhat detailed study of joy and sadness. After a description of the facts from a psychological and biological standpoint, the physiological, chemical, physical and mechanical sides of the phenomena are discussed and analyzed. Some experiments are also introduced. Some of his conclusions are, that in melancholiac depression there is a diminution of organic combustion, as is also the case in active melancholia; that in agreeable excitations there is an augmentation of combustion; that the weight of the body increases in joy and diminishes in sadness; that, in short, joy and sadness are indications on the psychical side of two quite different vital processes. No experiments are reported for the settlement of the question debated by Brugia on the one hand, and Mairet and Bosc on the other hand, as to whether the urine of melancholiacs is more toxic than that of the normal subject. Mairet and Bosc contend that the toxic element is proportional to the intensity of the disease, no matter what its form may be. The question of tastes and odors was also investigated. The author supports Lange in his statement that the phrase 'bitter sorrow' is more than a mere metaphor, the patients investigated suffering in periods of depression from an intense bitter taste, viscous saliva, dry tongue, etc., evidently due to general troubles of nutrition and buccal and stomachic secretions. Fetid odors tended also to disappear upon the alleviation of the depression. Bouchard has investi-

gated the chemical constituents of these odors, so observable especially in the insane. The muscular or mechanical aspect of emotions is also discussed, but nothing new or important is adduced. Experiments on the vaso-motor circulation, central and peripheral, tend to differentiate joy and sadness very clearly. The question of moral pain and sorrow as to its peripheral or central origin is also treated. No mention occurs of the splendid work of Sutherland ('Origin and Growth of the Moral Instinct').

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Zur Kenntniss des Einflusses einiger psychischer Zustände auf die Athmung. D. ISENBERG and O. VOGT. *Zeitschrift für Hypnotismus*, 1900, X., 131-158.

Ueber den Einfluss einiger psychischer Zustände auf Kniephänomen und Muskeltonus. O. VOGT. *Ibid.*, 202-218.

The point of view is that of individual psychology; each paper presents a careful study of a single individual—not the same individual, however, in the two papers. The first paper considers the effects on respiration of merriment (*heiter*), sadness (*traurig*), pleasantness (*angenehm*) and unpleasantness (*unangenehm*). These emotions were sometimes awakened by appropriate sensations, sometimes by imagination. Merriment increased the expansion of the chest and the extent and rate of the respiratory movements. Sadness had the opposite effects. Pleasantness decreased the expansion of the chest and decreased the rate, but increased the extent, of the movements. Unpleasantness had the opposite effects. Thus the effect of merriment is more like that of unpleasantness than like that of pleasantness, while the effect of sadness more resembles that of pleasantness.

As to the knee jerk and muscle tonus, Vogt finds them always changed in the same direction. Merriment (aroused by witty stories, etc.) caused a great increase in both; sadness (aroused by imagining a sad situation) a strong decrease; concentrated salt solution, disagreeable and stimulating, a moderate increase; sugar solution, agreeable but a little stimulating, a slight increase; pain (electricity) a moderate increase; a pleasant 'letting one's self go' produced a strong decrease; light hypnosis a decrease; intellectual work (mental arithmetic, etc., without anxiety as to correctness of result) caused a strong decrease, while muscular work (dynamometer) caused a strong increase; also, at the instant of cessation of muscular effort, a strong increase was noted. The author emphasises the facts that mental and

muscular work have opposite effects; and that merriment, though producing opposite effects to sadness, has the same sort of effect as the disagreeably stimulating; whereas sadness and the agreeably relaxing have like effects.

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MOTOR PHENOMENA.

Ueber die motorische Einstellung. LAURA STEFFENS. Zeitsch. f. Psych., Bd. XXIII. 1900. Pp. 241-308.

These experiments continue and amplify a research begun by Müller and Schumann. These authors (Pflüger's Archiv, 1889) found that if a light and a heavy weight be alternately lifted for a number of times, the muscles become adapted to the particular degree of effort necessary for raising each; and if then a weight perceptibly heavier than the lighter, but much lighter than the heavier, of the original two be substituted unexpectedly in the place of the heavier, it is raised with unusual rapidity and regarded as weighing less than the lighter weight; similarly, one lighter than the heavier weight would be regarded as weighing more than it.

Miss Steffens confirms these observations and adds a number of new details. Such motor adaptation, induced in the organs of one side of the body, does not affect the judgment of comparisons executed by aid of the organs of the other side. In case two different motor adaptations are induced in succession, *e. g.*, one for the succession light-heavy and the other for the reverse order or for weights approximately equally heavy, the two adaptations gradually fade away, meanwhile mutually opposing one another, and the older of the two disappears more slowly than the younger, if both are of equal strength. These adaptations often persist for 24 hours or more. Their presence was tested by measurement of the actual rapidity of the lifting-movements, as well as by conscious judgments as to relative weight. A given number of alternate liftings are more effective in establishing an adaptation when spread over a considerable time than when all are performed in quick succession; and a given number, spread over equal times, are more effective when divided into a larger number of smaller groups than when divided into a smaller number of larger groups. In these laws of their behavior the motor adaptations closely resemble psychical memories.

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Automatic Movements of the Larynx. H. S. CURTIS. American Journal of Psychology, XI., 237-239.

A tambour, applied to the skin over the larynx, registered its movements during silent reading and during mental repetition of a speech or poem. The records showed that movements took place in 15 out of 20 subjects; in some they were very small, in others nearly as large as the movements recorded during actual whispering. Those persons who made no movements during silent reading usually made none even during whispering. The author seeks to connect his work with the sound-focusing experiments of Hansen and Lehmann, as showing that imagined speech is not absolutely silent, but is accompanied by actual movements of the vocal organs.

Experiments on the Control of the Reflex Wink. G. E. PARTRIDGE. American Journal of Psychology, XI., 244-250.

A small rubber-faced hammer was made to spring towards the face and strike on a glass plate directly before the eye. The subject strove to avoid winking. His success was measured (inversely) by the number of winks made before inhibitory control was fully secured. Great variability appeared among the adults tested; some soon learned the trick, while others never gained full control. Alcohol made it easier to inhibit the reflex. About 1150 school children were similarly tested. The ease with which the control was secured increased rapidly from 5 to 7 years of age, and after that more slowly up to 12-14 years. No correlation could be made out between the child's success in this experiment and his school standing or the time of day. The means by which the wink was prevented seemed to consist largely in the preliminary tension of opposing muscles. Young children at first showed general muscular tension over the whole body, but this decreased after practice.

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De la relation entre le développement des canaux semi-circulaires et la coordination des mouvements chez les oiseaux. J. P. LAUDENBACH. Jour. de Physiol. et de Pathol. génér., I. Pp. 946-949. 2 plates.

Expériences sur l'anesthésie des canaux semi-circulaires de l'oreille. G. GAGLIO. Arch. ital. de biol., XXXI. 1899. Pp. 377-397.

Laudenbach's article is a brief account of some dissections of the

semicircular canals of birds with different motor habits. The two plates accompanying the text show the appearance of the organs in our most common birds—hen, duck, goose, pigeon, hawk, buzzard, owl, crow, heron, etc.

The author assumes that the greatest development of the coördination of movement is evident in birds which fly much, alight often, and have arboreal habits. This class of birds, he finds, is endowed with the canals of greatest size, and he concludes, accordingly, that 'there exists a direct relation between the development of the semicircular canals in birds and the degree of skill which they exercise in the coördination of movement necessitated by the struggle for existence.'

It is well known that any abnormal stimulation or irritation of the semi-circular canal is followed by characteristic motor phenomena. Various investigators have accounted for these motor disorders in two ways, viz.: that they are due: (1) to a disturbance of function, or (2) to the irritation of the nerve endings in the canals. To determine which of these hypotheses is correct was the object of Gaglio's research.

The chief method of previous investigators has been the destruction of one or more of the canals and a subsequent examination of the motor adjustment of the animals experimented upon. Deductions from such an experiment are open to the objection that the results are explained equally well by either of the two above hypotheses. The present research was an attempt to rule out one of the hypotheses. For this purpose the author has applied cocaine to the canals. He assumes that the cocaine will prevent the functioning of any sensory nerves in the canals in the same manner that it acts as an analgesic.

The results of the experiments are as follows: (1) The effects of the anesthesia of the canals, by application of cocaine, are equivalent to those which are observed after section or destruction of the organ. (2) When cocaine is applied to the cut organ the motor troubles, manifested after the lesion, remain. (3) The movements of nystagmus endure after application of the anesthetic, and, moreover, the cocaine applied locally never calms the nystagmus but reinforces it.

These phenomena tend to show, according to its author, that the "characteristic motor disorders consecutive to section of the canals are due to a suppression of function, and not to a reflex action determined by sensory and traumatic irritation."

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FATIGUE.

Ermüdungsmessungen. C. RITTER. *Zeitschr. f. Psychol.*, XXIV., 2. Pp. 401-444.

The author of this unnecessarily lengthy paper is director of the Ellwangen Gymnasium. A desire to find a measure of intellectual fatigue, applicable to classes as a whole, led him to make the measurements here reported upon.

The Griesbach method having been shown to be unreliable, he turned to those used by Ebbinghaus (*Zeit. f. Psy.*, Vol. 13, p. 401 ff.) and selected, to put it to a test, the 'memory' method, although Ebbinghaus had discarded it as not yielding the desired results. It consists in the dictation of a series of numbers to be reproduced in writing immediately after by the subject. Ritter gave it up on finding that series of words were in every way preferable to number-series. The most serviceable word-series he found to be of six two-syllabic nouns, each one accented on the first syllable. The words are to be so chosen that they do not make sense. For the highest class of the gymnasium and for adults he thinks that series of seven two-syllabic nouns, or of five nouns of three syllables, may be preferable. He trained himself to read the series each time in exactly the same way—same rapidity, same rhythmic intonation, etc., and always gave the students a preliminary trial. Two or three series were used for each test. His conclusion from a total of about 140 tests—two successive dictations being counted for each individual as one test—is that this method gives satisfactory results; it is better than all the others with which he is acquainted.

Professor Ritter tried also, but without result, the substitution of sentences for meaningless sequences of nouns. Then, 'mit Unermüdlichkeit,' he devised another method—Professor Ritter does not seem to know that this other method has been frequently used, if not in the study of fatigue, at least for other purposes—the subject is required to underline certain classes of words or certain letters in a piece of printed matter. At first a task was assigned, but it was soon found preferable to set, instead, a time-limit. The results of about 130 tests are summarized as follows: "On the whole, the figures warrant the claim that the underlining of letters and words is also a really practical test of fatigue," but he prefers dictation to underlining, not only because it is much more convenient, but chiefly because of the practice—again not to be completely avoided with the latter. Notwithstanding his apparent success Professor Ritter does not believe

that he has perfected his methods sufficiently to warrant the drawing of conclusions as to the comparative fatigue-producing power of the different branches of study—and for this he is to be commended.

The great advantage of a dictation and also of an underlining method over the æsthesiometric is that one person can test a whole class in a few minutes, and also that several of the sources of error which make the Griesbach method worthless are avoided. But the decisive question is evidently whether or not they are reliable means of measuring intellectual fatigue. This cannot be held to be proven by Professor Ritter; his investigation is not extensive enough, the results are not sufficiently uniform and there are too many contradictory figures to be accounted for on more or less satisfactory ground, or which elicit the remark, "Hier stehe ich vor einem Räthsel."

The fundamental weakness of all these recent attempts at discovering a practical means of determining fatigue is their great deficiency in precision. The only procedure that will give scientific satisfaction, it seems to us, is one in which the value of the tested method will be determined by comparison, not with the expected fatigue produced by one, two or three hours of this or that study, but with an amount of work actually measured. If, for instance, the work done had been the addition of figures according to the method developed by Kraepelin and used with complete success by Lindley in his research, 'Ueber Arbeit und Ruhe,' *Psychol. Arbeiten*, III., pp. 482-534, the chief cause of the dubiousness of Ritter's work would have been removed, as the amount of work done could then have been exactly computed. In this way one could secure in a comparatively short time results having a definite, incontrovertible meaning.

Another methodological criticism must be made: The use of lists of meaningless words offers a very serious difficulty in that it is impossible to make them equally difficult. Similarly the underlining of, for instance, *t* (*T*), is not *a priori* to be supposed, as Ritter does, equally difficult with the underlining of *s* (*S*). There is here, we believe, sufficient disparity to account for divergencies as great as those shown by Professor Ritter's figures. A preliminary investigation should have established the comparative value of the several lists of words and of the several underlining tasks; this could be done by the means suggested above.

There are several other avoidable minor sources of error in these investigations.

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L' influence de l'âge sur quelques phénomènes de la fatigue. A. MAGGIORA. Arch. ital. de biol., XXIX. 1898. Pp. 257-286.

This article gives us a comparison of the muscular ability of the author at different ages. Maggiora, it appears, continued the work begun by Mosso, and has given us in these twenty pages the results of single experiments with the Mosso ergograph at intervals of about three years. The first experiments were made at the age of twenty-four and a half, the last experiments were made at the age of thirty-five. A comparison of fatigue curves with various weights at the varying ages shows, according to the author, an ability increasing directly with age. To have the muscles thoroughly recover from the effect of making a single fatigue curve, it was found necessary to have rest periods of two and of one and one-half hours, respectively, for the earlier and later days. From the few experiments described the author concludes that "the neuro-muscular apparatus increases in ability from youth to old age." No daily variation is noticed, the conclusions seem to be drawn from insufficient data, and the results have only a suggestive value.

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ETHICAL.

Psychic Rudiments and Morality. GEORGE E. DAWSON. American Journal of Psychology, Vol. XI. Pp. 181-224.

The aim of this article is to give a scientific account of the genesis of immorality, with a view to discovering the most effective means of dealing with this troublesome and dangerous phenomenon. The discussion is judicious, and while little originality is claimed for it by the author, it renders familiar views more precise, gives them the support of facts and of accepted theories, and thus strengthens them greatly as a basis for practical measures.

After a preliminary discussion of differentiation, or the development of specialized organs as the normal process of evolution, Dr. Dawson points out that the elimination of organs that cease to be useful is an essential part of the process. Useless organs which are not eliminated, at least to the point of becoming rudimentary, stand in the way of the development of the specialized organs that characterize the next stage of evolution. Moreover, there is elimination in this sense as well of psychic qualities as of physical organs, and it occurs alike in ontogenetic and in phylogenetic history.

Dr. Dawson's central thesis is that immorality is essentially due to imperfect elimination of psychic rudiments. In other words, the psychic development of immoral individuals was arrested, either temporarily or permanently, and qualities of character that were useful somewhere in the line of their human or animal ancestry, and that recurred in their own developmental history, failed to be eliminated, thus blocking the way of higher qualities, and throwing their characters out of gear and out of adjustment with their social environment. Many examples of uneliminated organs and psychic qualities are cited by the author. If the nomadic, and feral instincts generally, are not eliminated, individuals are likely to become tramps and paupers; if the predatory instincts and defective sense of property rights characteristic of all animals and savages persist, a thievish nature is the outcome; if development is arrested at the point where the sexual passions are of animal or savage strength, sexual offenders of more or less repulsive types are the result.

In some cases of arrested development there is merely temporary or permanent persistence of subcivilized or animal characteristics in otherwise healthy individuals; in others there is hypertrophy or disease of these abnormally persistent qualities; while in the most serious cases the presence of unwholesome characteristics has the effect of destroying the equilibrium of the moral nature, which is on a high plane of evolution, and peculiarly unstable.

This explanation of immorality, whether it exist in a single direction or pervade the whole nature, as the result of imperfectly eliminated psychic rudiments, is, to the best of the present reviewer's knowledge, the most plausible and scientific so far offered.

But the article also suggests means of treating immorality and immoral individuals, based upon the methods of elimination employed by Nature.

And first, much childish and youthful unruliness and even immorality is due to merely temporarily incomplete elimination of lower traits. Here there is hope. Nature is on the side of elimination, and by observing her methods wholesome moral character will be evolved.

Secondly, childhood and adolescence are periods upon which the stress of moral training should be placed. During the first the craving for food, uncontrolled emotions, strong egoism, etc., are to be dealt with. "The dangers of the second period center in the awakening sex-consciousness."

And coming to methods of elimination, (1) Nature never extirpates an organ or a quality. Atavistic qualities are gradually evolved,

and they must be slowly developed out of existence. Sane education is patient; extirpation is in every case emasculation. While touching on this point, Dr. Dawson scarcely does it justice. At times, by giving an undesirable quality full play, its period of evolution and dissolution is shortened. This view is based on ample evidence, and it can be held without accepting the wild oats theory.

2. But Nature makes use of starving as well as of ample feeding to reduce undesirable organs. Sometimes an organ or quality is allowed to atrophy from lack of opportunity to come into play. "Deliver us from temptation" is at times as fitting a prayer for immature children as for adults. The truth is that the two complementary methods of feeding and starving no-longer-useful characteristics are employed with equal effect by nature, and both should be employed in education. Unfortunately, at present, tact and judgment have little aid from science in determining which method should be employed in any given case; investigation of this point is much needed. Dr. Dawson's plan is unduly simplified through neglect of the importance of giving too persistent qualities full play.

And here it is appropriate to point out that Dr. Dawson's article might well be supplemented by a discussion of one of Nature's favorite devices. In animals, and especially in man, many pairs of mutually antagonistic propensities are implanted to hold each other in check. Fear and trust in chicks, calves, and other young animals furnish the classic instance. It follows that one natural and scientific method of bringing undesirable qualities under control is to develop their antagonists. These antagonists deserve study, especially the organized group of the more reflective among them, which has the name of conscience.

3. The method of transformation of function needs little comment. The familiar pedagogical principle of emphasizing the interesting here finds its foundation. Especial emphasis is laid by the author on the fact that by this method the adolescent sex-consciousness may be healthily developed in domestic, altruistic, artistic and religious directions.

4. Finally, Dr. Dawson shows that Nature destroys unduly abnormal and harmful individuals, or at least prevents them from propagating their kind. And man has learned to remove the vicious and criminal from society, and to prevent them from perpetuating their stock. But much more thorough and effective measures are here called for, especially in the case of habitual criminals and incurables generally. Of course, Dr. Dawson is aware that this method of

elimination can be employed only under the most carefully devised safeguards.

Articles of this type will ultimately place in society's hands scientifically forged weapons with which to combat its chief inner foe.

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ÆSTHETICS.

The Origins of Art: A Psychological and Sociological Inquiry.

By YRJÖ HIRN, Lecturer on Æsthetic at the University of Finland. London and New York, Macmillan & Co. 1900.

This important volume has undertaken the considerable task of bringing into some system the partial theories which, one after the other, have in recent years added to the interest if not to the lucidity of æsthetic inquiry. The genetic method, with its belief in utility-selection as an ultimate criterion of rationality, has been so prolific in hypotheses as almost to discredit itself, and this struggle of theories has brought us to a point where any attempt at unification is welcome.

Although the book before us cannot be looked upon as in any sense final, there being, as I think can be shown, too many subsidiary questions left unanswered, the method and temper of the writer ought to commend themselves to all those who realize the breadth and depth of the æsthetic problem. For, in the first place, Hirn does not hesitate to face the problem of the fundamental and undervived nature of the art impulse, and he insists that "even if all other hypotheses be banished, æsthetic research cannot possibly dispense with the fundamental assumption of the unity of art." Although art in its beginnings is not wholly 'autotelic' and disinterested, its present nature requires an explanation which shall relegate particular utility conceptions to a secondary rôle and find in some intrinsic function of the psycho-physical organism its unitary origin.

This unitary origin of the art impulse is to be found in emotional expression—on the one hand for the enhancement of pleasure and on the other for the relief of pain. This expression is in turn conditioned and modified by subordinate social utilities, such as the motives of information, sexual and religious propitiation and stimulation, and magical efficiency through the production of illusion (p. 301).

The revival of the idea that art is essentially expression, bringing the author in line with the best idealistic traditions as Mr. Bosanquet has shown them to us, is in no sense, however, a return to what some

would call unprofitable things. Careful study makes it clear that the method is entirely 'von Unten nach Oben' and to understand what this expression of emotions rather than ideas is, we are led through a careful psychological study.

The fundamental law of hedonic expression, that all pleasurable experience tends to express itself in movements of expansion while painful experience has corresponding movements of withdrawal and inhibition, may be called the primary law of expression. Except in certain cases, where variation can be easily explained, this law holds good of the most complex emotions, as well as of simple algedonic states, whatever our theory of emotion may be. And the weight of evidence is, moreover, in favor of the theory that not only does pleasure thus become associated with movement, but that, within limits, movement is itself a source of pleasure. To this primary law of functional enhancement and arrest must be added therefore the secondary form of motor expression, namely, a tendency to automatically increase pleasure by motor function and to relieve pain by the pleasurable functioning of the expression of painful states. This distinction between primary and secondary expression of the emotions is fundamental for Hirn's theory of art, a distinction which, as he well says, has the merit of alone explaining the pleasure gotten out of painful emotions, a phenomenon which has made itself striking even in social manifestations, such as the crying feasts of the Maoris and the ceremonial wailings of ancient Greece—a distinction, too, which alone makes explicable the contradictory statements of James that sorrow is increased by sobbing and in another place that 'dry and shrunken sorrow is more painful than any crying fit.' In a brilliant chapter Hirn has also made clear the function of apparently pathological and painful motor expression as a secondary form of expression for the relief of more deep-seated and painful inhibition.

Out of such fundamental laws of the psychophysical organism emerges an automatic and autotelic activity of self-expression, upon which the secondary laws of selection can act. But, to quote the author's own words, "as against the 'Spieltrieb' theories it was objected that play never develops of itself into art, so it may now be objected, with as much reason, that all immediate or secondary emotional expressions, however interesting they may be, give us no information on artistic manifestations." And it is indeed a question why formless expression should not have sufficed for algedonic enhancement and relief; why order and form, the very essence of the æsthetic, should necessarily have appeared. It is to my mind doubtful whether the

author's answer to the question which he himself has raised, is adequate. No new principle is necessary, he insists, for we need only apply these same laws found valid in the emotional life of the individual, to the larger emotional life of the race. Sympathetic imitation is the medium for the communication of feeling. These widening 'somatic resonances' from individual to individual tend, in the first place to the enhancement of emotion, and thus further the ends of expression, and in the second place to the objectification and exteriorization of the emotion in such forms as make it socially communicable, or facilitate coöperation in emotional expression. We may agree with him as to the importance of sympathetic imitation, both in its 'outer' and 'inner' form for all kinds of social objectifications. It is probable that the first form of objectification in art is the expression of 'objectless feeling' in the three logically most primordial arts or lyrical forms, gymnastic dance (ordered movement of the body), geometric ornament (ordered movement of the eyes) and simple rhythmic music or singing, without melody. No less primordial, doubtless, is the mimetic expression which slips into the earliest forms of dancing and singing, thus adding to moods the particular emotional nuances, which in time find their way into the formative arts. All this we may grant, the essential contagiousness of rhythm and its function in furthering social cohesion and expression, as brought out in the researches of Wallaschek and Bücher—and yet we may well ask ourselves at this point the question whether the sociological conceptions here introduced are sufficient to bridge over the chasm between mere unæsthetic movement and the ordered expression of art. If there is in the individual consciousness nothing to explain why mere instinctive movements did not suffice, it is not clear how social imitation has answered the question more satisfactorily than the play impulse of Groos, acting upon instinctive movements.

In fact, the whole question of the nature of imitative transmission, its mechanism, the material which it transmits (witness the divergence of opinion of Tarde and Baldwin as to whether ideas or emotions are imitated), indubitable as the facts themselves may be, is so uncertain as to make the principle singularly unsuitable, of itself, to explain these phenomena. It is probable that the secret of the passage of merely impulsive expressional movement into purposive artistic expression is not to be disclosed by this sociological principle, but must be found rather in a closer study of the æsthetic attention, in the 'Verinnerlichung' of motor impulses of which Lange and Groos have made so much. Hirn indeed seems to recognize this, for he remarks: "When a savage

has attained so high a state of development as to be able to control the impulse to dance and yell for joy the first dithyramb has been composed." Just what this state is, when the selective attention turns from the object itself to an inner imitation and coördination of its own reactions, just what its significance is in the volitional and emotional life, is a question that still remains unanswered. Some insight into the phenomenon itself Hirn has certainly given us by his beautiful illustration from the Bacchic Candelabra in the Louvre, where we see, in the same orgiastic dance, some of the dancers still seeking relief in violent unæsthetic movements, while others have found it in the gentle rhythmic motion of the æsthetic dance.

For the final test of the theory of emotional expression Hirn leaves the genetic and historical method and the psychological analysis of the art impulse for an examination of the 'Work of Art' itself. The traditional origin of classical art in the Dionysic festivals and an introspective study of the creative impulse of to-day would seem to give an easy answer to our problem. But in view of the historical uncertainty of the former, probable as it is from other points of view, and the difficulty of getting definite and credible results in regard to the latter (although in this connection it seems curious that, after mentioning Ribot and Paulhan in his bibliography, the author makes absolutely no use of their interesting researches in a direction which would tend to support his own theory), it seems better to turn to an analysis of the concrete work of art itself.

With Hirn's thesis in regard to the principle of unity in concrete works of art the modern critical sense doubtless agrees. It is emotional rather than intellectual, and the *émotion fixe*, to use Ribot's term, is the dominating and selective principle in both creation and appreciation of most works of art. The bold intellectualism of Hegel, Vischer and Taine and the scientific method of the formalists both fail to satisfy the modern æsthetic sense. "The desire to fix a passing emotional state—in order to facilitate either the revival of the same state or its transmission to others—by the help of intellectual elements"; 'to avoid the transience of feeling' is undoubtedly the *raison d'être* of most artistic effort, as Ribot has learned from his artist questionnaire; and Hirn has made a point of considerable importance when he shows that, not only throughout the range of art itself, but even in the æsthetic appreciation of nature, it is emotion that selects the focal point and makes all other elements subordinate to it. And yet, in spite of the author's fine feeling for artistic values and his critical faculty which makes us realize the truth of his general

point of view, the chapter as a whole remains unconvincing. There is still a *desideratum* much to be desired, from the scientific point of view. What is an emotion detached, objectified? What is the mechanism of subsumption of subsidiary ideas and emotions under the emotional abstract or mood? Is there 'revival' and 'transmission' of an emotional state as such? These problems of 'emotional memory' and of a '*logique émotionnelle*' are involved in every line of the chapter under discussion, as they must underlie every theory of art or emotional expression. The facts of artistic expression may require these notions, and the present writer is inclined to think they do; but, in any case, their position calls for discussion at the present time.

The second portion of the volume, which discusses the concrete origins of art and the effect of secondary non-æsthetic motives upon its development, is an unusually good illustration of the proper use of the data of ethnology. Although it is doubtful whether any but experts should pronounce judgment in this field, yet to the careful reader its main contentions seem to have been made out with more than usual probability. The skillful use of the facts of primitive art to relegate these utility factors to their proper secondary rôle is a distinct contribution to æsthetic theory and ought to clear the air considerably. "Just as the play impulse is a concrete source of art and the chief classes of art forms are already foreshadowed in the various forms of playful experimentations, and as this simple activity furnishes sufficient bulk of raw material of which the art impulse when once developed can then avail itself," so the utility factors can furnish material for the art impulse, but are not its origin.

Especially important are the chapters which deal with the relation of art to sexual selection and propitiation. It is shown that the Darwinian conception of æsthetic attention and judgment on the part of the female (to be sure only guardedly suggested by Darwin but raised to a principle by his successors) is but a round-about way of explaining the facts of sexual selection. Not only can the supposed æsthetic factors, which have developed in the male of animals, be explained as merely secondary sex and species characteristics, but even in primitive man the means resorted to 'to attract by pleasing,' self decoration, for instance, may be genetically explained, without any reference to æsthetic judgment, as secondary sexual and tribal marks, having direct stimulating power. The difficulties in the way of reading æsthetic judgment into animal consciousness, which Groos has also seen and called attention to in his '*Spiele der Thiere*,' are no greater than those involved in connecting the æsthetic judgment in primitive men too

closely with sexual selection. It is probable that the Darwinian emphasis upon the erotic origin of art applies not so much to primitive man as to the special social conditions of barbaric peoples. In any case, striking as the influence of sex upon art development has been, Hirn seems to be justified in considering that influence much overestimated, and to explain all art by the principle is impossible in the light of the results of later ethnological investigation.

In his effort to disclose all the non-æsthetic and secondary motives of art the author has come upon an important connection of art with life—through the belief in magic. Here is at least a suggestion for the genetic explanation of the baffling 'moment' of illusion in all art. Psychopathic magic, resting as it does upon the idea that the material contiguity and even similarity of the fetish to the object of desire gives power over the real object itself, has, doubtless, through the exteriorization of the emotional and volitional values of the original, been the source of much of the reality feeling that envelops primitive art. It is, however, Hirn points out, only the principle of similarity in magic that has had any real influence upon art. It is significant, too, that the 'volts,' *i. e.*, the dolls and drawings used by primitive peoples to bewitch, are not in any realistic sense similar, nor indeed are they intended to be, for the vaguest resemblance seems to suffice. This vague similarity seems rather to suggest a fantastic, but still natural, belief of the primitive mind in the invisible connection of things similar. Illusion in its full sense is neither expressed nor sought for. One fact about volts, idols and magical symbols in general, extremely favorable to Hirn's emotional theory of art, and which brings the illusion of magic into close relation with that of art, the author fails to bring out, *viz.* : that however unlike the original they may be they emphasize some emotionally important aspect of the thing or person, which single aspect, just because of its singularity, attracts the attention in a semi-hypnotic fashion and makes it possible to spread the emotion of reality over the entire object no matter how crude it may be. In fact, too much detail here or in the higher developments of art may disturb the illusion.

One is tempted to say in this connection that the passage from the illusion of magic to the conscious self-illusion of art is just such a problem as faced the thinker in an earlier portion of the work in connection with the derivation of purposive ordered emotional expression from mere emotional reaction, and it is only just to the author to say that he does not profess to have given more than a suggestion of kinship. His purpose was, in fact, merely to show how the unæsthetic

motive of a common belief in magical efficiency may have contributed to the development of the 'illusion' aspect of æsthetic expression.

In concluding our study of what is in many respects an important book, it is worth while to emphasize again the critical care and insight with which the author has sought to separate the æsthetic from the non-æsthetic motives in art. It should be noted, too, that the insufficiencies in the treatment of the main problem which it seemed necessary to point out, are just such insufficiencies as are incident to a method which distinguishes too sharply between the problems of 'origin' and 'meaning.' Unless the present meaning of æsthetic experience be taken as a starting point in the investigation of origins, it is doubtful whether one will find more than vague 'kinships,' to use the author's own expression, between primitive forms of emotional reaction and specialized æsthetic experience. Hirn's problem is one of origins, and his theory of the genesis of art, to the extent that it does justice to the problem of art's meaning, has overcome the one-sidedness and fragmentary nature of preceding theories. On the other hand, those who cling to the purely causal point of view may decide that he concerns himself too much with 'meanings.' It is hard to serve two masters!

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EXPERIMENTAL.

L'acuité stéréoscopique. B. BOURDON. *Revue Philosophique*, January, 1900. Pp. 74-78.

Helmholtz's experiments on the accuracy of the binocular perception of depth led him to the conclusion that the just noticeable difference is about equal to the limen of distance between two images in monocular vision—that is, in the neighborhood of sixty seconds. Stratton's work with the pseudoscope indicates a considerably greater degree of accuracy in the depth perception. M. Bourdon here gives an account of some experiments with the Helmholtz needle method, where, however, the movements of the middle needle were not watched by the subject, who judged of its position each time after it had been brought to rest—a better method, because 'the perception of continuous change is always less delicate than that of discontinuous changes.' The results show that the just noticeable difference is very much smaller than that obtained in Stratton's experiments, the minimum being an angle of about five seconds. The author suggests that the correlate of this value in monocular vision is not the just noticeable distance between two images, but the least perceptible difference in

the size of two images. What corresponds to the former in binocular vision is rather the least distance apart at which double images can be observed. Further experiments by M. Bourdon show this distance to be approximately 100", which he considers sufficiently close to the 60-90" representing the limen of distance between two images in monocular vision.

L'asymétrie sensorielle olfactive. E. TOULOUSE and N. VASCHIDE. *Revue Philosophique*, February, 1900. Pp. 176-186.

The absolute limen for the smell of camphorated water was determined for a large number of subjects, each nostril being tested separately. Out of sixty-four persons, fifty-six gave the limina both of sensation (recognition of a smell in general) and perception (recognition of camphor) lower for the left nostril. In explanation, the authors point to the probability that there is no crossing of the fibers of the olfactory nerve, the connection of the more important fibers at least being with the hemisphere on the same side of the body. Thus a superiority of the left nostril in smell would be due to the same cause that produces superiority of the right side in other senses. Ferrier, and especially Collet, are quoted in support of the hypothesis that there is no decussation of the olfactory fibers. Further, along with superiority of the left nostril for smell, there was a superiority of the right nostril for touch sensations, tested by solutions of ammonia, in the fifteen persons examined to decide the question. And the subjects who showed asymmetry in favor of the right nostril for sensations of smell were all either left-handed or ambidextrous.

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Aspects of Mental Economy. An essay in some phases of the dynamics of mind, with particular observations upon student life in the University of Wisconsin. By M. V. O'SHEA. *Bulletin of the University of Wisconsin*, No. 36. April, 1900.

Professor O'Shea gives in this Bulletin the results of an investigation into the dietaries and methods of life of the students of the University of Wisconsin. The author claims that the primary object in asking the students to report was to direct their attention to these matters. The author enters, however, a wider area of discussion when he enters the subject of mental economy in general. He pleads for an 'energetic' conception and treatment of the relation of mind and body. The essay is largely made up of citations from different authors on matters of nutrition. The relative values of foods in the

production of nervous energy, the preparation of foods, hours for meals, individual peculiarities in digestive capacities, expense of dietaries, exercise, sleep, muscular and nervous waste, etc., are some of the chief topics treated. The style and often the subject-matter are quite popular in their nature. Perhaps they were so intended. This may explain the hortatory nature of some of the concluding remarks. Such phrases as, "Tell me what company you keep and I will tell you what you are," and "The M.D.'s have not yet dealt largely with the subject of nutrition for healthy beings," etc., are probably meant for popular distribution. The work is valuable for the information it contains.

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PHYSIOLOGICAL.

A Note on the Significance of the Small Volume of the Nerve Cell Bodies in the Cerebral Cortex in Man. HENRY H. DONALDSON. *Journal of Comparative Neurology*, Vol. IX., No. 2. 1899. Pp. 141-149.

The substance of this paper by Professor Donaldson, of Chicago University, may perhaps be given in the words of his own Summary:

"(1) The weight of all the nerve cells in the human encephalon is less than 27 grams [although in number many more than 9,200,000,000]. (2) When comparison is made of human encephala grouped according to race, sex, mental power, stature and age, the differences in weight, within each group, are always more than twice that of the nerve cell bodies, and hence these differences depend mainly on variations in the medullary substance. (3) Small variations in the mass of the nerve cell bodies (though physiologically highly important) escape detection by the method of weighing, or may be masked by the greater growth of the medullary substance."

Certain other considerations proposed by the author in the course of his paper have psychological interest, which perhaps warrants their repetition here: "We infer from these observations, and from the fact that the dendritic branches of the cortical cells become more numerous as we ascend in the vertebrate series, that the principal means of increasing the *physiological complexity* of the cortex is represented by the dendrons of the cortical cells, together with the associated terminals surrounding them. As those portions appear to furnish the structural basis necessary to the exhibition of intelligence, variations in their number and extent may be fairly correlated with the variations in mental power. * * * The neurone may be resolved into

three portions. First, the receiving portion; second, the conducting; third, the transmitting. These correspond, first, to the cell body and its dendrons; second, to the axone; and third, to the ends of the branches of the terminals. By means of the first portion, the neurone is rendered responsive to nerve impulses brought into its neighborhood; by means of the second, impulses arising in the cell body are conducted to the ends of the axone; and by means of the third, the terminals, these impulses stimulate other neurones. It is therefore on the dendrons and the terminals, *i. e.*, the receiving and transmitting portions of the neurone, that the complexity of the encephalon is dependent." From this paper it may be seen why it is that the brain of an adult often weighs no more than it weighed when only six years old.

A Plethysmographic Study of the Vascular Conditions during Hypnotic Sleep. By E. C. WALDEN. Amer. Jour. of Physiol., Vol. IV., No. III. July 1, 1900. Pp. 124-161.

The object of this research was, simply, to determine if the arm's volume increases from suggestion and during hypnosis as it does during normal sleep. There are reported the results of twenty-five experiments on hypnotic sleep and a like number on suggestion. The instruments employed were Howell's plethysmograph with Bowditch's recorder and a modification of Masso's sphygmomanometer—the latter for recording the blood-pressure.

There was found regularly a comparatively brief and sudden diminution in the circumference due to constriction of the peripheral blood vessels. When suggestion ended the arm expanded from 0.2 to 10.0 cc., lasting from 1 minute to 2 hours. After slowly expanding thus the arm gradually constricted up to the end of the hypnosis, sometimes 3 cc. At the instant of awakening a sudden but brief constriction occurred, 'due to the action of mental and sensory stimuli upon vasomotor centers' during the process of awakening; relaxation to normal then rapidly occurred. Just after the suggestion there was a fall in arterial pressure in the fingers of about 7 mm. of mercury. The pulse-rate was slower during hypnosis, especially at first, followed after waking by an abnormal rapidity, which continued 15-20 minutes. The breath-rate likewise was slower, especially at first, lasting several hours; at times it was above normal. There was a steady, although very slight, fall of rectal temperature during the hypnotic sleep, followed by a maximum rise of 0.4°. The surface temperature of the arms was higher during hypnosis than at the experiment's beginning.

It gradually rose during the first hour about 0.6° , then gradually fell about 0.8° . Suggestion during the waking state caused a decrease in the volume of the arm, as the plethysmographic tracing demonstrated; this was followed by an increase. 'A pronounced and increasing vaso-constriction in the arm during most of the sleep-period' was then the most positive and suggestive result obtained.

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PATHOLOGICAL.

Les causes psychologiques de l'aboulie. BARON C. MOURRE. Rev. Phil., L., 3. September, 1900. Pp. 276-285.

M. Mourre considers aboulia the inability to transform an idea into its act as the consequence or product of victory among antagonistic ideas. The aboulie person is conscious that she wishes to do a thing, and at the same time quite aware that she does not do it. Opposed to the motor idea is always its inhibitory opposite, and it is this latter tendency that triumphs. Why it triumphs the greater part of this brief article attempts to explain.

The causes of 'psychic paralyses' the author deems of two sorts. Some of them arise in ideas of acts different from those intended by the agent. Others, on the other hand, arise in the fear of the subject that she will be 'paralyzed' if she attempts the act. Of the latter class, in part, are cases of aboulia, the patient realizing and fearing her infirmity. In true aboulia the agent considers her will free, and she fails to act because she chooses not to act, on account of her fear of failure. But this association by contrast is not the sole nor primary cause of aboulia. There is, second, a diminution of the sensations and of the desires, making the pleasurable motogenic balance smaller than in normal organisms. The causes of this diminution, or even loss, of desires to action are almost always contrarieties, chagrins which occasion a profound moral depression, making life distasteful. This state is by no means incompatible with an excess of emotional activity, as is frequently experienced by those who have to do with this class of mental derangement. But effort seems useless, because its usual rewards are absent.

"The psychological cause of aboulia will be then the difficulty of voluntary effort, and the physiological cause an organic trouble of the brain of which the localization is unknown." Although no novelty, such an explanation has a certain usefulness and probably much of the truth in it so far as it goes.

GEORGE V. N. DEARBORN.

TUFTS COLLEGE.

NEW BOOKS.

Report of the U. S. Commissioner of Education, 1898-99. Vol. II. Washington, Gov. Printing Office. 1900. Pp. viii + 1249-2578.

Proceedings of the American Association for the Advancement of Science, New York Meeting, June, 1900. Easton, Pa. 1900. Pp. xcv + 409.

Socrate. C. PIAT. Grands philosophes series. Paris, Alcan. 1900. Pp. 270. Fr. 5.

Saint Augustin. L'Abbé J. MARTIN. Grand philosophes. Paris, Alcan. 1901. Pp. xvi + 403. Fr. 5.

Hume's Enquiry Concerning the Principles of Morals. Religion of Science Library. Chicago, Open Court Publ. Co. 1900. Pp. 169.

Kant. TH. RUYSSSEN. Grands philosophes. Paris, Alcan. 1901. Fr. 5.

La Foule criminelle. S. SIGHELE. 2^{me} Éd. Paris, Alcan. 1901. Pp. 300. Fr. 5.

In this new edition of the French translation of Sighele's well-known and influential book the author protests against the use made of his work without credit by certain authors, notably by M. Le Bon. In this he should have the sympathy of his readers; for the case is a flagrant one: Le Bon has had circulation and citation in English without reference to the work of Sighele from which he drew much of his material. The present work is revised and enlarged. J. M. B.

Psychologie appliquée à la morale et à l'éducation. F. RAUH and G. REVAULT D'ALLOUNES. Paris, Hachette. 1900. Pp. 320.

A text-book for use in the *enseignement secondaire des jeunes filles*. As we should expect from the pen of M. Rauh it utilizes the latest results. J. M. B.

Un Siècle: mouvement du monde de 1800 à 1900. By a committee: President, Monseigneur Péchenard. Paris, Ouden. No date. (5^{me} mille.)

A review of all the intellectual movements of this century from a Catholic point of view.

Le vocabulaire philosophique. E. GOBLET. Paris, Armand Colin. 1901. Pp. xiii + 489.

L'organisation de la science. L. FAVRE. Paris, Schleicher Frères. 1900. Pp. lx + 409.

- La Folie: ses causes, sa thérapeutique au point de vue psychique.* TH. DAREL. Geneva, M. Reymond et Cie.; Paris, F. Alcan. 1901. Pp. 196.
- La musique des couleurs.* L. FAVRE. Paris, Schleicher Frères. 1900. Pp. xiv + 113.
- The Human Nature Club.* EDWARD THORNDIKE. New York, London and Bombay, Longmans, Green & Co. 1901. Pp. viii + 235.
- Problems of Evolution.* F. W. HEADLEY. New York, T. Y. Crowell & Co. 1901. Pp. xvi + 373.
- Le problème de la vie.* Essai de sociologie générale. LOUIS BOURDEAU. Paris, Alcan. 1901. Pp. xii + 372.
- Essai critique sur le droit d'affirmer.* ALBERT LECLÈRE. Paris, Alcan. 1901. Pp. 264.
- Experimental Psychology: A Manual of Laboratory Practice.* EDWARD BRADFORD TITCHENER. Vol. I., Qualitative Experiments. Part I: Student's Manual. New York, The Macmillan Co. 1901. Pp. xx + 214. Price, \$1.60.
- Wörterbuch der philosophischen Begriffe und Ausdrücke.* DR. R. EISLER. 3, 4, 7 u. 8. Lieferungen. Berlin, E. S. Mittler u. Sohn. 1899-1900. Pp. 193-288, 289-384, 705-956.

NOTES.

It gives us much gratification to announce that Professor William James has completed his first course of Gifford lectures and intends to read them at Edinburgh University in May. He has been in England during April, much improved in health.

PROFESSOR J. MARK BALDWIN has gone abroad (sailing April 6) in the interest of his *Dictionary of Philosophy and Psychology* and other literary undertakings. He and his family remain in Italy until about June, and summer in Switzerland. (Constant address: Care Controller, University Press, Oxford, England.) He and Professor Wenley of Michigan are among the delegates from the United States to the Ninth Jubilee of Glasgow University.

PROFESSOR R. B. JOHNSON, of Miami University, has been called to Ohio State University, at Columbus. He will enter upon his duties there in the autumn.

WE are requested to state that, owing to continued dissatisfaction with the administration of the Department, the following professors of the Faculty of the School of Pedagogy of New York University announce their resignation from the University: Samuel Weir, professor of history of education and ethics; Edward F. Buchner, professor of analytical psychology, and secretary of the faculty; Charles H. Judd, professor of experimental psychology.

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